ABSTRACTS

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

December 5, 1850.

LIEUT.-COLONEL SABINE, R.A., V.P. & Treas. in the Chair.

A paper was read, entitled "Researches into the Structure of the Spinal Cord." By Jacob Lockhart Clarke, Esq. Communicated by Samuel Solly, Esq., F.R.S. Received October 15, 1850.

The author having undertaken a series of observations with the view of determining, if possible, the relations which appear to subsist between the spinal nerves and the respiratory nervous centres, was led into a more extended inquiry than he at first contemplated, the results of which are communicated in this paper. After stating that the observations were made, by means of a microscope of the best construction, upon many thousand preparations of the spinal cord of Man, of the Calf, Sheep, Pig, Dog, Cat, Rabbit, Guinea-pig and Frog, he describes the methods adopted in making these preparations. The following are the results of his observations.

At the lower extremity of the spinal cord, the posterior gray substance consists of a single mass, formed by the coalescence of the two posterior horns, and consequently the gelatinous substance extends across the middle line. The anterior gray substance, however, is divided by the anterior columns into two distinct horns, which curve inwards and taper to a rounded point. The spinal canal is large and nearer the anterior surface of the cord.

The nerve-vesicles are scattered irregularly through the gray substance.

The fibres of the gray substance are of the tubular kind, and of small average diameter. There are two classes with regard to direction, transverse and longitudinal. Of the transverse, there are two orders, the antero-posterior and latero-transverse.

The antero-posterior are continuous with the posterior roots of the

nerves, and probably with the anterior roots. The latero-transverse stretch between the opposite sides of the gray substance, forming its transverse commissure.

Longitudinal Fibres.—Those of the gelatinous substance, have the characters of minute tubular fibres, presenting dark outlines and frequent varicosities. Their average diameter is the $\frac{1}{12,000}$th of an inch. They have no nuclei as generally stated, and have no resemblance to the sympathetic fibres or caudate processes. These fibres are found through the rest of the gray substance, but in smaller numbers; immediately below the gelatinous substance, however, they form a remarkably dense band.

Changes in the form of the gray substance are observed on examining the cord from below upwards. The posterior mass is gradually divided into two, and the gelatinous substance is interrupted in the middle line. Two vesicular masses gradually appear at the sides of the spinal canal, traversed by fibres from the posterior nerves, and from the transverse commissure. The anterior horns also undergo certain changes, become club-shaped in the lumbar region, and contain large groups of caudate vesicles. In the middle of the dorsal region the posterior horns have again coalesced, and the gelatinous substance again crosses the middle line. The anterior horns are straight and narrow, and their vesicles are scattered through them irregularly. In the cervical enlargement the posterior gray substance has become again divided to its greatest extent into two horns, and the gelatinous substance is again interrupted in the middle. The two vesicular masses from the sides of the spinal canal are here, as in the lumbar enlargement, included in the posterior horns. Here also, and in the lumbar enlargement, the anterior horns are nearly similar in appearance.

The nucleus of vesicular substance, in which the spinal accessory nerve may be seen to arise, has been traced by the author as low down in the cord as the upper part of the lumbar enlargement.

The grayish structure immediately surrounding the spinal canal, consists chiefly of fibro-cellular tissue, and is not to be regarded as a commissure, as maintained by Stilling and Foville. The spinal canal, as stated by the former observer, extends through the whole length of the cord.

The vesicles of the cord are found chiefly in the anterior horns, as usually stated, but occur also in the dark masses situated in the dorsal region at the sides of the spinal canal; and also more sparingly in the posterior horns as far as the gelatinous substance. The author has never been able to make out satisfactorily in mammalia, any connection between the nerve-vesicles and the tubular nerves, nor between the latter and the caudate processes.

The blood-vessels of the cord enter through the anterior and posterior median fissures, through the smaller fissures in the white columns, and at the roots of the nerves. They form a beautiful network of loops along the whole periphery of the gray substance.

Of the White Columns of the Cord.—The anterior columns have no proper transverse commissural fibres, but are crossed horizontally

but chiefly obliquely, by tubular nerve-fibres and blood-vessels, which proceed from the gray substance on each side and decussate in front of the spinal canal: nor are the posterior white columns connected by any commissural fibres, the posterior fissure reaching down to the gray substance.

Origin of the Spinal Nerves.—The posterior roots are attached exclusively to the posterior columns. Their fibrils generally are finer than those of the anterior roots. The anterior roots are attached to the anterior parts of the antero-lateral columns, which they traverse horizontally in straight bundles, till they reach the anterior horns, in which they break up and form a complicated network. The author has not yet been able to determine whether any of the fibres of the spinal nerves ascend with the longitudinal white columns.

December 12, 1850.

SIR FREDERICK POLLOCK, Lord Chief Baron, V.P., in the Chair.

The following letter from M. Arago to Lieut.-Col. Sabine was read, and ordered to be entered on the Minutes.

"Paris, Dec. 8, 1850.

"MON CHER MONSIEUR,—Mon age, ma mauvaise santé, l'état déplorable de mes yeux, et la part que j'ai dû prendre aux évènements dont mon pays a été le théatre depuis le 24 Février 1848, m'avaient fait supposer que j'étais entre dans cette période de la vie où rien ne peut produire une vive impression. Votre lettre ma détrompé. La nouvelle que la Société Royale avait bien voulu m'accorder pour 1849 et 1850 la médaille de Rumford m'a comblé de joie. Veuillez être aupres de vos honorables confrères l'interprète de mon inaltérable reconnaissance; dites leur surtout, que leur indulgence me fera redoubler d'efforts pour que les travaux qui me restent à publier ne soient pas indignes de la faveur dont j'ai été l'objet.

"Agréez, mon cher Sabine, l'expression de tous mes sentiments.

"F. ARAGO."

A paper was then read, entitled, "On the Action of Nitric Acid on various Vegetables, with a more particular examination of *Spartium scoparium*, Linn., or Common Broom." By John Stenhouse, Esq., F.R.S. Received November 18, 1850.

This paper is a continuation of a series of investigations intended to elucidate the nature of vegetables by means of chemical reagents. A preceding paper contained an account of the effects produced by the action of sulphuric and hydrochloric acids on the *matière incrustante* of several plants belonging to different great classes of vegetables. The effects of nitric acid upon a variety of vegetable groups are now described; the researches having been undertaken in the hope that by means of this powerful reagent some light might perhaps be thrown on peculiarities in their respective constitutions.

The first plant experimented on with this view, was the *Populus balsamifera*, as representing the numerous family of the Poplars. A quantity of the smaller branches of this tree, cut into pieces, was exhausted with boiling water. The dark-coloured bitter-tasted liquid which it yielded was evaporated to the state of an extract, which was digested for nearly twenty-four hours with dilute nitric acid. This strongly acid solution was evaporated to dryness on the water bath, the dried residue was dissolved in a considerable quantity of hot water, and the solution after cooling was carefully filtered. The clear liquid, after concentration to a very moderate bulk, was exactly saturated with carbonate of potash. A yellow crystalline sediment quickly appeared. It consisted of nitropicrate of potash, and the potash salt of a new acid, to which the author has given the name of *nitropopulic acid*. The mixed potash salts were then collected on a filter, dried by pressure, and were treated with a cold solution of dilute carbonate of potash, which readily dissolved out the nitropopulate of potash, while it left the nitropicrate of that base unacted on. The two salts were then separated by filtration, the nitropicrate remaining on the filter, while the nitropopulate was dissolved in the alkaline liquid. The solution was next slightly supersaturated with hydrochloric acid, when the nitropopulate of potash precipitated as a compact crystalline powder, which was purified by repeated crystallizations out of hot water. When pure, the potash salt, which crystallizes in small lemon-yellow prisms, is boiled with a considerable excess of hydrochloric acid, and, on the cooling of the solution, the nitropopulic acid is deposited in silky needles, forming concentric groups of a pale yellow colour. By digestion with animal charcoal, the nitropopulic acid is rendered perfectly colourless. It is very soluble in water, and still more so in weak and strong spirits of wine. By digestion with strong nitric acid, it is rapidly converted into nitropicric acid. In several of its characters nitropopulic acid bears a considerable resemblance to indigotic acid. When subjected to analysis, the formula of nitropopulic acid dried in the air, was found to be $C_{14} H_3 N_2 O_{13} + HO + 2Aqu$; that of the acid dried at 212° F., $C_{14} H_3 N_2 O_{13} + HO$.

The potash, silver, soda and baryta salts were also analysed.

The *Populus nigra*, when treated with nitric acid, was also found to yield nitropopulic acid, which appears therefore to be characteristic of the poplar tribe.

An extract was also prepared from *Salix russelliana*, or the Bedford-willow, which, when it was digested with dilute nitric acid, yielded a great deal of oxalic and nitropicric, but no nitropopulic acid. Extracts of the *Cytisus laburnum*, or the laburnum-tree; of the *Swetenia mahogani*, or the mahogany-tree; of the *Pyrus malus*, or the apple-tree; of the *Cratægus oxyacantha*, or the hawthorn; of the *Ribes nigrum*, or the black currant bush; of the *Betula alnus*, or the alder; of the *Ulex europæus*, or the furze; of the *Calluna vulgaris*, or common heather; of the root of the *Curcuma longa*, or turmeric; of the seeds of the *Bixa orellana*, or annotto; of the *Sambucus nigra*, or the elder; of the *Cytisus scoparius*, the *Spar-*

tium scoparium of Linn., or common broom, when treated with dilute nitric acid, also yielded oxalic and nitropicric acids. The extracts of *Quercus robur*, or common oak, and of *Betula alba*, the birch-tree, when digested with nitric acid, only yielded oxalic acid, but no nitropicric or any analogous nitrogenated acid.

The results of these experiments seem to indicate therefore that a far greater number of plants are capable of yielding nitropicric acid than has generally been supposed, those which fail to do so constituting a very small minority.

As the extract of broom, *Spartium scoparium*, besides yielding nitropicric acid, exhibited some interesting peculiarities, it was subjected to a more minute examination. When an aqueous decoction of broom was concentrated to about a tenth of its bulk, and set aside in a cool situation for twelve hours, it gelatinized into a greenish-brown coherent mass, which was thrown upon a filter and washed with a little cold water. It consisted chiefly of a yellow crystallizable colouring matter (scoparine, the diuretic principle of broom), which was at first contaminated with a considerable amount of chlorophyl. It also contained a small quantity of a volatile organic base (sparteine, the narcotic principle of broom), the greater portion of which however remained in the mother-liquors.

The scoparine, when purified by repeated crystallizations out of hot water and spirits of wine, formed pale yellow prisms of a feeble acid character. Its formula was found to be $C_{21}\ H_{11}\ O_{10}$. Scoparine acts as a powerful diuretic.

The mother-liquor from the crude scoparine, after having been concentrated to a very moderate bulk, was distilled along with a considerable excess of soda, when a colourless basic oil slowly collected at the bottom of the receiver. This oil was the volatile base sparteine. It has a strongly alkaline reaction, completely neutralizing the most powerful acids. It is but slightly soluble in acids, but readily dissolves both in alcohol and in æther. It boils at about 550° F. The taste of its salts is very bitter. The formula of this base is $C_{15}\ H_{13}\ N$. The nitrate, sulphate and hydrochlorate of sparteine are exceedingly soluble, and crystallize with great difficulty. The nitropicrate of sparteine forms long, slender brittle needles, undistinguishable from nitropicrate of potash. Its formula is $C_{15}\ H_{13}\ N$, HO, $C_{12}\ H_2\ N_3\ O_{13}$.

The double chloride of platinum and sparteine crystallizes in rectangular prisms. This salt contains two equivalents of water, which it loses at 266° F. Its formula, when dried at 212° F., is $C_{15}\ H_{13}\ N$, $HCl + Pt\ Cl_2 + 2HO$.

The double mercurial salt crystallizes in large, right rhombic prisms, exhibiting the diamond lustre. Its formula is $C_{15}\ H_{13}\ N$, HCl, $Hg\ Cl$.

Sparteine appears to be a strong narcotic poison, though much inferior in this respect to either nicotine or coneine.

It is plain, therefore, that in employing a decoction of broom-tops

in dropsical affections, as has hitherto been the practice, the patient is subjected to the narcotic influence of the sparteine, as well as to the diuretic effects of the scoparine, a result which in general is not likely to be desirable. The author does not, however, think it is at all necessary to employ chemically pure scoparine for medical purposes. If a decoction of broom be evaporated to dryness on the water-bath, then treated with a little dilute hydrochloric acid, the mixture thrown upon a filter and washed with a small quantity of cold water, almost the whole of the sparteine will be removed, and the dark green gelatinous mass remaining on the filter will be found to possess the diuretic without the narcotic properties of the plant.

December 19, 1850.

SIR BENJAMIN C. BRODIE, Bart., Vice-President, in the Chair.

Captain Lefroy, R.A., was admitted into the Society.

The following communication received from Professor Hermann Schlagintweit was read:—

"Results of Observations on the Distribution of Temperature in the Alps."

1. The greatest irregularities and the most considerable local inflexions of isothermals are observed at the lower heights.

2. Generally speaking, the depression of temperature is also very sensible at stations in the Alps when proceeding from south to north; and the eastern parts are colder than the western. One degree of latitude produces in the plain of Lombardy a difference of temperature amounting to $0^{\circ}{\cdot}7$ C. In the interior parts of the Alps the difference is $0^{\circ}{\cdot}5$ to $0^{\circ}{\cdot}6$ C.

3. If we consider the isothermal lines on a longitudinal profile of the Alps, we find that their forms show some connexion with the mean elevation of the different parts of the Alps. The isothermals rise where the mean elevation is greater; they sink at the borders and on smaller groups. This convexity of the isothermal lines in the centre of the Alps is still more considerable if we represent by them the temperature of the earth, since the latter is still more intimately connected with the mass of the mountains, with the insulation and radiation of the rocky substance; whilst for the temperature of the air, differences of that kind are more eliminated by its mobility.

4. The vertical distances of two isothermals are the greatest near the base of the Alps, attain afterwards a minimum, and become in the higher parts again a little greater. The position of the minimum takes place in the Northern Alps and the group of the St. Gothard at nearly 6000 French feet; in the central parts at nearly 7000; for the group of Mont Blanc it seems to be at a still greater elevation.

5. The height corresponding to a depression of temperature of

1° C. is in the mean 540 F. feet = 90 toises = 166 metres, if we compare the lowest stations of *continued observation* with the highest stations; but if we consider the temperature of the highest *summits*, the height becomes rather less (510 feet for the Central Alps).

6. The mean temperature of the air seems to be, for the highest summits, from −13° to −15° C.

7. At the greater elevations the temperature of single months is generally altered in this manner:—The temperatures of February and January, of August and July, differ less from each other than they do at lower stations.

8. The influence of the general form of the surface on the temperature is particularly evident when we consider the mean temperature of the months. The valleys during the winter are in general colder than the mountains, the cold air sinking down and being accumulated in them; during the summer they are comparatively warmer, the heat being reflected and radiated by the insulated masses near them, and circulation produced, especially in the horizontal direction: their climate is therefore subject to greater extremes, though in the annual mean it scarcely differs from the Alps in general. The declivities during the winter are comparatively warmer, since the air near the surface, after sinking down in the valleys, is replaced readily by less cold strata. During the summer, particularly in southern exposures, and if the relative height above the bottom of the valley is not great, they are also a little warmer, since then they can partially be reached by the ascending current of air. But this increase of temperature being smaller than that of winter, these situations have a more constant climate than the valleys. The mean temperature of the year on declivities, particularly with southern exposures, is therefore a little higher than the mean of the Alps in general.

9. Summits and declivities, with an exposure to north and to northeast, show also the character of a constant climate; but the temperature of summer is much lower, and consequently the annual mean is also sensibly depressed.

10. The depression of temperature with elevation is greater in summer than in winter, amounting for example to 1° C. for 440 feet in July, and for 710 feet in January. The cause of this is, that in the lower parts of the mountains the differences between single months are greater than in the higher parts.

11. The elevation of the point, near which on a vertical line the depression of temperature is the greatest, is a different one in every month. It is the highest, when the Alps are covered with snow, in December and January; from March to September this point is generally found near the limit of snow; in October and November it lies below the snow-line.

12. The height of the snow-line in the months does not coincide always with the same isothermal. In January the snow-line and the isothermal of 0° are both nearly on the base of the Alps; but from this time to July and August, the isothermal of zero moves

quicker upwards than the snow-line, and from August to January quicker downwards. The snow-line therefore in the first period coincides with isothermals warmer than 0° C.; in July it is even at +5° C. The snow-line, in the usual sense, that is to say its highest limits in summer, is, at the mean temperature of the year, −4° C.

13. Over large masses of snow and glaciers there is remarked, particularly on fine days, a descending current of air (glacier-wind), which has a great influence on the general depression of temperature near the limits of snow.

14. The absolute extremes of cold on single days are at the lower stations sometimes so great, that they are comparatively but little surpassed by those on the higher points. But the differences between the higher and lower part are much greater if we consider the maxima of heat. The absolute maxima seem scarcely ever to exceed 5° or 6° C. on the highest summits of the Alps. On all days the decrease of temperature is greater at the time of the maximum than at the minimum.

15. Compared therefore to the temperature of high latitudes, the *summits* of the Alps correspond nearly to 70° N. Lat. But the climate of the highest elevations on the Alps is much less severe than that of Northern Asia, and is more constant than that of Polar America. Their minima of winter are much surpassed by nearly all stations in northern latitudes; but the maxima of summer are colder than those of nearly all points on high latitudes at little elevation above the sea.

A paper was also in part read, entitled "On the Exogenous Processes of the Vertebræ." By Professor Owen, F.R.S. &c. Received November 8, 1850.

The Society then adjourned over the Christmas holidays, to meet again on the 9th of January, 1851.

January 9, 1851.

Lieut.-Col. SABINE, R.A, V.P. and Treasurer in the Chair.

The reading of Prof. Owen's paper "On the Exogenous Processes of Vertebræ" was resumed and concluded.

The author commences by a definition of these as contradistinguished from the autogenous parts or 'elements' of a vertebra, and exemplifies them by instances from Human and Comparative Anatomy, which show the necessity of a distinct substantive term for each of such parts and processes. The terms proposed are as follows:—

	Names.	Synonyms in Comparative Anatomy (Cuvier).	Synonyms in Human Anatomy (Soemmering).
Autogenous.	Centrum.	Vertebral body.	Corpus vertebræ.
Autogenous.	Neurapophysis.	—— laminæ.	Arcus posterior vertebræ, seu radices arcus posteriores.
Autogenous.	Pleurapophysis.	—— rib, cervical rib, hatchet-bone.	Costa, seu pars ossea costæ, processus transversus vertebræ cervicalis.
Autogenous.	Hæmapophysis.	Sternal rib, chevron-bone.	Cartilago costæ.
	Parapophysis.	Inferior transverse process.	Radix prior seu antica processus transversi vertebræ cervicalis.
Exogenous.	Diapophysis.	Superior transverse process.	Radix posticus processus transversi vertebræ cervicalis, processus transversus.
Exogenous.	Zygapophysis.	Articular or oblique process.	Processus obliquus seu articularis vertebræ.
Exogenous.	Metapophysis.	Prolongation of articular process.	Duo processus accessorii processui transverso et articulari superiori interpositi.
Exogenous.	Anapophysis.	Supplemental articular process.	
Exogenous.	Hypapophysis.	Inferior spinous process.	
	Spine { neural.	Spinous process.	Processus spinosus.
	Spine { hæmal.	Inferior spinous process.	

The principal aim of the present communication was to point out the proportion of the vertebrate animals in which the metapophysis, anapophysis and hypapophysis were present, their principal modifications, and their title to the distinct appellations bestowed upon them.

The metapophysis is noticed by Monro, in 1726, as a small rising between the roots of the superior oblique and transverse processes; and both this and the anapophysis appear to be defined in similar terms, as sometimes characterising the lumbar vertebræ, by Soemmering. The author of the present paper commences his comparative anatomy of both processes by describing them in the European, Polynesian and Australian varieties of the human race. He then passes to the Quadrumana, and traces their modifications and progressive development in the Chimpanzee, Orang-utan and Gibbon, in the *Cercopithecus ruber*, *Semnopithecus entellus*, *Macacus rhesus*, *Macacus niger*, *Macacus nemestrinus*, *Papio mormon*, *Ateles paniscus*, *Ateles Beelzebuth*, *Cebus capucinus*, *Callithrix sciureus*, *Lemur nigrifrons*, *Lemur Catta*, *Lichanotus Indri*, and *Stenops gracilis*.

In the order *Carnivora*, the same processes are described in the Lion, Hyæna, Wolf, Fox, Civet, Genette, Otter, Sable, Kinkajou, Mydaus, Badger, Bear and Seal. The presence of anterior articular processes (pre-zygapophyses) is demonstrated in all these Carnivora, in the anterior dorsal vertebra; and their apparently greater production in the succeeding vertebræ is shown to be due to the gradual transference of their articular surfaces upon the metapophyses, which are processes distinctly superadded.

In the Rodent Order, the modifications of the metapophyses and anapophyses are described in the common and Malabar Squirrels, the Marmot, the Hydromys, the Rat, the Cape Jerboa, in which the anapophyses attain their maximum of relative size; in the Beaver,

the Porcupine, the Coypu, the Paca, the Capybara, and in the Hare. The distinction between the metapophyses and the pre-zygapophyses is particularly strongly marked in the Capybara.

In the Insectivora, the Hedgehog is an instance in which metapophyses are developed but not anapophyses. The modifications of both processes are traced, in the Marsupial Order, in the Thylacine, the Dasyure, the Wombat, the Perameles, and in two species of Kangaroo. The diapophyses being developed from the last as well as the antecedent dorsals in these marsupials, renders the homology of the transverse processes of the lumbar vertebræ unmistakeable: but the diapophyses of those vertebræ are lengthened out by anchylosed pleurapophyses, of which those of the first lumbar vertebra in the Wombat, compared by the author, had not completely coalesced. In the *Ornithorhynchus paradoxus* the metapophyses are double in some of the anterior dorsal vertebræ, but become single in the sixth dorsal, and gradually increase to the twelfth. The anapophyses are rudimental.

In the Ruminant Order, the accessory processes are described in the Elk, the Giraffe, the Gnu, the Equine Antilope, the Ox, the Aurochs, the Camel, the Vicugna, the Memmina, and the Musk-deer. The anapophyses are rarely, and then only very feebly developed; the metapophyses are constant; they appear as tubercles above the diapophyses in the anterior and middle dorsals, and pass upon the zygapophyses in the penultimate or last dorsal. The author records a peculiarity in the skeleton of a musk-deer (*Moschus moschiferus*). In the pair of ribs attached to the thirteenth dorsal vertebra the tubercle is wanting, and the diapophysis is obsolete, as in the last dorsal vertebra of other *Moschidæ*; but in the following vertebra, answering to the first lumbar in other *Moschidæ*, the rib is developed with a head and a distinct tubercle, articulated to an equally distinct diapophysis. This plainly demonstrates the homology of the diapophysis in the next vertebra, answering to the second lumbar in other *Moschidæ*.

There are no anapophyses in the Hog-tribe and Hippopotamus: the metapophyses resemble those of the ruminant artiodactyles. The perissodactyle Ungulates manifest some peculiarities. In the Sumatran Tapir, *e. g.* the metapophysis is a very distinct process in the third dorsal, subsides in the four next dorsals, and reappears as a prominent process in those that follow, but does not attain the position upon the zygapophysis except in the last lumbar vertebræ. In the Horse and Rhinoceros, as well as the Tapir, although there are no proper anapophyses, the diapophysis of the last lumbar developes an articular surface on its back part which articulates with a corresponding surface on the sacrum. In the vertebræ of the Elephant a peculiarity is pointed out which is not adverted to by Cuvier or De Blainville, and appears to have escaped notice, viz. an accessory pair of joints between the metapophysis and anapophysis, commencing between the seventeenth and eighteenth dorsals, and continued to between the first and second lumbar vertebræ. The metapophyses have been undescribed, also, in the *Cetacea*, although

they are represented in the plates of the 'Ossemens Fossiles' distinctly from the anterior zygapophyses, and exist in many vertebræ after these processes with their articular surfaces have wholly disappeared: the modifications of the metapophyses, and their mode and place of superseding the prezygapophyses, are described in the *Delphinus Tursio* and *D. Delphis*: their modifications are also pointed out in the Dugong. But the most remarkable development and complexity of the accessory exogenous processes is presented by certain members of the Order *Bruta* or *Edentata* of Cuvier. The author commences with a description of them in the Sloths, and gives his reasons for considering the length of the neck in the three-toed species to be due to the superaddition of two cervicals between the dentata and eighth vertebra, which, from certain characters of its complex transverse process, he regards as homologous with the sixth cervical vertebra of the two-toed species.

In the Cape Ant-eater (*Orycteropus capensis*), both metapophyses and anapophyses are present on the eighth dorsal vertebra; the former are continued to near the end of the tail, the latter subside in the last lumbar. In the armadillos the metapophyses commence abruptly about the middle of the back, and progressively increase until they equal the long neural spines in height: they develope two articular surfaces, one on the inner side of their base, another on the outer side: the latter articulates with the anapophysis, which is remarkable for its thickness, and developes a second inferior articular surface for the parapophysis, which, together with the diapophysis, is developed from all the lumbar vertebræ. These complex joints are illustrated by drawings taken from two species of Armadillo.

The exogenous processes present still greater complexity in the true Ant-eaters. The metapophyses commence in the cervical region, change their place from the zygapophyses to the diapophyses in the anterior dorsals, and back again to the zygapophyses in the posterior dorsal and lumbar vertebræ, where they supersede those processes; and develope accessory articular surfaces for the anapophyses. These not only present an upper articular surface for the metapophysis, and a lower one for the parapophysis, but develope a third outer one for a new articular surface upon the diapophysis; so that, were not the ordinary articular processes, or zygapophyses, obliterated in the posterior dorsal and lumbar vertebræ, there would be not fewer than eighteen synovial joints, in addition to the intervertebral joints, in the posterior lumbar vertebræ of the Great Ant-eater. These processes and articulations are illustrated by figures taken from the Great Ant-eater; and the necessity of the substantive names for the processes, and of adjectives to signify their added articular surfaces, was exemplified in the explanation of those figures. The peculiar complexity of the vertebræ of the Edentata having been, previously to the investigations of the author, illustrated by a comparison with those of the Serpent tribe, he next enters upon the question of the precise nature and extent of this analogy, and shows that, although the complex joints in both are comparable to the tenon-and-mortice joints in

carpentry, they are produced by different processes in the Mammal and the Reptile. The zygapophyses exist in both; to these, in the Mammal, are superadded the joints developed on metapophyses and anapophyses, which are *below* the zygapophyses; but in the Serpents, the superadded joints are on parts which the author terms the 'zygosphene' and 'zygantrum,' and are *above* the zygapophyses. Some characteristic differences are next pointed out in the Ophidian genera *Coluber*, *Hydrus*, *Naja*, *Crotalus*, *Python*, and the extinct genus of large serpents from British eocene strata called *Palæophis*. The author also points out that the tenon-and-mortice joints are not, as was supposed, peculiar to the Ophidian reptiles, but exist in the Iguana, where they are likewise due to the superaddition of zygosphenal and zygantral articulations.

The author finally enters upon the comparative anatomy of the 'hypapophysis,' that name being applied to the process, commonly exogenous, from the under or ventral surface of the centrum, rarely autogenous from the same aspect of the capsule of the notochord. The modifications of the hypapophysis are exemplified in the Hare and Rabbit, the Cape Jerboa, the Hydromys, the *Phoca grœnlandica* and *Leptonyx serridens*, in the *Hippopotamus*, the *Megaceros*, the Musk-deer, the Camel, the Giraffe, and other Ruminants. In the *Ornithorhynchus* the atlas is remarkable for a pair of hypapophyses, like the first vertebra in the Sudis or *Arapaima gigas*: but the most remarkable instances of the development and modification of the hypapophysis are to be met with in the class of Birds. It is there well-marked in the anterior cervical vertebræ, especially in the dentata, and reappears in the lower cervicals as a pair of processes, which defend and sometimes encompass the carotid arteries, forming a quasi-hæmal arch, as in the Pelican. The still more extraordinary developments of the hypapophysis in the *Aptenodytes* and *Sphæniscus* are specially described and illustrated by figures. The modifications of the same process are pointed out in some extinct Reptilia, as *e.g.* the *Crocodilus basifissus*, the Mososaurus, the Iguanodon and the Ichthyosaurus: in the latter the hypapophysis is exogenous in the neck, as in some Lizards, and forms the so-called 'wedge-bones:' the part usually called 'body of the atlas' is serially homologous with these; the true centrum of that vertebra being the so-called odontoid process. The memoir concludes with a demonstration of the serial homology of the hæmal arches of the tail, sometimes called chevron-bones, and the essential distinction of the hypapophyses from the hæmapophyses, and at the same time from the parapophyses, with which the hypapophyses co-exist in the cervical and anterior thoracic regions of the Crocodile.

The paper is illustrated with fifty-five drawings, of which detailed descriptions are appended to the memoir.

A communication was read, entitled, "Researches on the Distribution of Vegetables in the Alps compared with the Differences of Climate, and on the Periodical Development of Plants at different heights." By Adolph Schlagintweit.

January 16, 1851.

Lieut.-Col. SABINE, R.A., V.P. and Treasurer, in the Chair.

The Chairman, by desire of the President, read the following letter:—

"Downing Street, Jan. 6, 1851.

"MY LORD,—I beg to inform your Lordship, that I shall set apart One Thousand Pounds, from the fund for Special Service, to be applied by the Council of the Royal Society in the same manner as the grant made for Scientific purposes last year.

"I have the honour to be,

"Your Lordship's obedient humble Servant,

"J. RUSSELL."

"*The Earl of Rosse.*"

The following papers were then read:—

1. "On the Results of Periodical Observations of the Positions and Distances of Nineteen of the Stars in Sir John Herschel's Lists of Stars favourably situated for the investigation of Parallax contained in Part III. of the Phil. Trans. for 1826, and in Part I. for 1827." By Lord Wrottesley, F.R.S. &c. Received November 14, 1850.

In Sir John Herschel's papers above referred to, he shows that if a double star occupy a certain position with respect to the ecliptic, and one of the components be supposed to be very much nearer to the earth than the others, a considerable periodical and parallactic change will take place in their angle of position, and that the maximum variations from the mean position will occur at two opposite seasons of the year, which indicate the best times of observation of the parallax of the star. Sir John gives a list of stars thus favourably situated, with the coefficients of the maximum parallactic variation of the angles of position, and the times of their occurrence subjoined.

Lord Wrottesley having erected at his seat in Staffordshire an observatory provided with a good equatorial, determined to devote the instrument to a good trial of the method, and the present paper contains the results of his observations and researches.

The equatorial employed was that formerly belonging to Mr. Beaumont. Its telescope is of 10 feet 9 inches focal length, and the object-glass is of $7\frac{3}{4}$ inches clear aperture, and a good glass of its size. The instrument is mounted according to the usual English method for a fixed observatory, viz. with a long polar axis resting in Y^s at each end. This polar axis is 14 feet 3 inches long, and 10 inches square in the middle, having pivots at the ends of hard bell-metal. The Y^s above and below are attached to massive stone piers, supported by a very firm and large foundation of brick-work. The steadiness of the instrument is not satisfactory, compared with that of some recently-established equatorials. The observations were made with a parallel-wire micrometer, containing one fixed and two

moveable wires, and the value of its scale was well determined both by Mr. Beaumont and Lord Wrottesley. The power usually employed was 450.

In the progress of the observations it was found that they were attended with considerable difficulties, chiefly arising from the impracticability of obtaining a sufficient number of observations at both the proper periods of the year, and also from the circumstance that many stars require to be observed at an inconvenient hour in the early morning, when the observer, fatigued by night observing, is unequal to the task. The fact of the difficulty of the observations is evidenced by the paucity of the trustworthy results, after more than six years' uninterrupted observing, viz. from February of 1843 to October of 1849. Of sixty-nine stars proposed for observation only forty-eight have been observed, and of them nineteen only have been observed at both periods of the year. The results of the observations of these nineteen stars are given in the paper.

Four tables are given, the first of which contains the separate results for each day's observations of every star, both for distance and angle of position, with the probable error and weight of each, computed according to the ordinary formula of the calculus of probabilities, and also with the assigned arbitrary weight of each, estimated according to the judgment of the observers. It contains also estimations of the magnitudes and colours of the stars for each night.

The second table gives the similar results combined for each period of observation, with the computed weights and probable errors.

The third table gives the combined results of all the observations for the main epoch of observation, together with the approximate R.A. and N.P.D., and the whole number of observations.

The fourth table gives the results for the separate epochs for those stars only which afford reasonable hope of the detection of parallax, four stars being omitted as evidently binary systems, and some others whose components were equal in magnitude, and the observations of which did not give any indication of parallax, being also omitted. The differences of the angles of position, as indicating parallax, are distinctly exhibited, first as resulting from such observations as were made at consecutive and opposite seasons, and secondly as resulting from the comparison of all the observations made at one period of the different years with all made at the other period.

In discussing the final results, the author remarks that only three stars, viz. 118 Tauri, 100 Hercules, and Herschel 95, were observed satisfactorily at the opposite and consecutive seasons, and these exhibit such discordances in the partial differences, that it seems necessary in all cases to depend only upon the average difference of position obtained by comparing all the observations at one period of the different years, with all at the other period. He finds that there are then only five stars in the list, viz. 32 Eridani, 41 Aurigæ, δ Geminorum, an anonymous star in Cancer, and Herschel 95, which show differences in the proper direction, and so much greater than the probable errors as to deserve much attention, as exhibiting parallaxes measurable by this method, and, of these, δ Geminorum

is subject to great doubt, from the smallness of the number of the observations at the late period of the year.

The results however are entitled to rather more consideration in this respect, that, with regard to the greater numbers of the stars that are physically unexceptionable (that is, omitting binary stars, and those whose components are of equal magnitude), the directions of apparent change of position are favourable to the supposition of a measurable parallax. This is particularly the case with respect to 32 Eridani and Herschel 95, which the author in conclusion recommends to the notice of astronomers provided with adequate instruments for observing them.

2. "Magnetic Survey of the Eastern Archipelago." By Captain C. M. Elliot of the Madras Engineers. Communicated by Lieut.-Col. Sabine, V.P. Treas. R.S. &c. Received Jan. 15, 1851.

In the year 1845, the Committee of Physics of the Royal Society having expressed a wish that a Magnetic Survey should be made of the East Indian Archipelago, Captain Elliot was ordered by the Court of Directors of the East India Company to undertake that duty, after the close of the Singapore Magnetic Observatory. The observations at that observatory were discontinued at the end of the year 1845, but the instruments were still allowed to remain, that the portable instruments might be occasionally compared with them during the Survey.

The object which the author proposed to himself was the determination of certain magnetic lines within the limits of the Survey: the lines of no dip, and of the maximum horizontal component of the earth's force; the minimum intensity of the total magnetic force; and finally, the line of no declination. He was also desirous of observing the variations of the magnetic elements, and of ascertaining whether the changes of the declination, of magnetic intensity, and of the barometer, were uniformly similar over so large an area. The fixed stations for this latter purpose were sixteen in number, and the time employed at each station varied from a few days to several months. They were spread over an area of 28° of latitude, and more than 40° in longitude, viz. from 16° latitude north to 12° south, and from 80° to 125° longitude east. This part of the globe coincides very nearly with the position of minimum total force. Of the sixteen stations, nine were to the south of this line, three to the north, and four in its immediate vicinity. Four stations were in the islands adjacent to Singapore; one in Borneo; one in the island of Java; two in Sumatra; one in the island of Mindanao; one in Celebes; one at the Cocos or Keeling Islands, which was the most southern station to which Capt. Elliot could venture; one at Penang, and one in its immediate vicinity; one at Nicobar, an island in the Bay of Bengal; one at Moulmein, which was the most northerly, and one at Madras, which was the extreme westerly, station. The total number of days employed in observing at the fixed stations amounted to 496. The instruments employed at the fixed stations were, for the changes of declination, sometimes three, but never less

than two; and for the changes of the horizontal force, a bifilar magnetometer with a thermometer for registering the temperature of the enclosed magnet; a barometer, either Newman's standard or a portable, with their attached thermometers; a dry and wet bulb thermometer for ascertaining the humidity of the atmosphere; and a standard thermometer. The observations were usually made under canvas, and close to the sea-shore. There being usually but one assistant, it was impossible to observe during the whole twenty-four hours; the hours of observation selected were therefore from 3 A.M. to 9 P.M. inclusive. In Borneo, and during the first four months in Java, there were three assistants, and observations were taken hourly.

As these observations were made at the different stations in different seasons, it was necessary to have the observations of some station for upwards of a year, for the purpose of instituting a comparison between the changes of one station with another. This was more especially the case with the declination changes, as the times of extreme westerly variation are continually altering. The curves of changes of declination at Singapore were therefore projected for each month of the three years 1843, 1844 and 1845, during which time the observations had been made hourly; and likewise for each of the four seasons; by comparing with these the observations made during the Survey, the author was able to distinguish the changes due to geographical position from those due to the season of the year.

At Singapore, the extreme westerly variation occurs in December at 19 hours, or 7 A.M.; in January at 20^h; in February at 21^h; in March, or the month in which the sun passes the equator, there is a slight retrogression; in April the extreme westerly variation is at 21^h; in July and August at 23^h; in September at 22^h; in October at 21^h, and in November at 20 hours. In the winter the extreme westerly variation is at 20 hours; in spring at 21 hours; in summer at 23 hours, and in autumn at 21 hours; agreeing in this respect with the spring curves, but differing very materially as regards the progression of the needle eastward in the afternoon. The oscillation has much the greatest range during the winter months; autumn comes next, and preserves an almost perfect parallelism with the winter. The spring and summer curves preserve their parallelism during the afternoon.

The mean curves of each of the three years have an almost perfect resemblance. There are two most decided maxima at 18^h and at 3 or 4 hours, and one minimum at 21^h. There are besides two other minima at 10^h and 17^h, and another maximum at 14 hours, making in all three maxima and three minima. If we turn to the seasons, we find in the winter only one strongly defined maximum and minimum. In spring two maxima, one minimum; in summer and autumn two maxima and two minima. The amount of oscillation is greater in winter than in summer. The sun is a longer time to the south of the line of minimum force than to the north of it. Singapore is in north latitude $1° 18'$, and the lines of no dip and minimum force cross the meridian of Singapore in latitude $8°$ north nearly, and

therefore the sun is to the south of these lines about 220 days of the year. If we compare the oscillations of the declination at the different stations with the Singapore curves, we see that of the sixteen stations two do not agree, one is doubtful, and the other thirteen do agree with the march of the needle at Singapore. Of the three not in accordance, the one that is doubtful is in the vicinity of the lines of no dip and minimum force, whilst the two which do not agree, viz. Madras and Moulmein, are both to the north of the line of no dip, and of the line of minimum force. The Moulmein observations were made in the month of April; the hours of morning maximum and minimum are 21 and noon, whereas at Singapore the hour of morning minimum is 21, which is that of the maximum at Moulmein. Again, the Madras observations were taken in the autumn, the morning maximum and minimum being at 20 hours and at noon; there is no well-defined morning maximum at the same period at Singapore, but the morning minimum occurs at 21 hours. We see, therefore, that the stations south of the line of no dip and of minimum force, with the exception of Keemah in the Celebes, all are in perfect accordance with the march of the needle at Singapore. Sambooanga, in the island of Mindanao, is nearly on the line of no dip, and agrees generally with the Singapore curve; but it is to be observed, that as we proceed northerly and approach the lines of no dip and of minimum force, the similarity to the Singapore curve becomes much more faint, although agreeing tolerably well. This may be seen on inspection of the Plates, where the curves of Sumatra, Java and the Cocos, which are extreme southerly stations, agree exceedingly well with those at Singapore.

The next observations referred to are those of the horizontal force at different seasons. As the observations at the different stations were made at different times of the year, it appeared necessary, as in the case of the declination, to ascertain what changes might take place in the different seasons at a station where an uninterrupted series had been observed for a considerable time. Accordingly, Singapore, where observations were made hourly during three years, was chosen as the point to which all the stations of the Survey were referred. Unlike the declination, which changes its time of extreme western position, not only in every season, but in nearly every month of the year, the maximum horizontal force at Singapore occurs with great regularity either at 22 or 23 hours, whilst the minimum at 9 or 10 hours is but very faintly defined. In other respects also the curves preserve a perfect parallelism, if we except only the greater range during the spring.

In the curves representing the changes of the horizontal force at the different stations of the Survey a general similarity is perceptible; a very decided maximum at noon, with a minimum very faintly marked. The irregularity, where observable, appears to be principally attributable to the short time during which the observations were taken. At Moulmein, the large oscillation appears to be principally due to the very great change of temperature. The coeffi-

cient of temperature of the magnet was determined at Moulmein, and yielding the same results at other stations, is considered satisfactory as regards accuracy. The range of the thermometer at Moulmein was upwards of 35°; and in the middle of the day, in the tent, the heat was almost insupportable, being upwards of 105°. The oscillation of the horizontal force is much greater at Moulmein than at any other station. The minimum oscillation is at Singapore, and increases both northward and southward. During the Survey the portable bifilar was compared with the large Observatory bifilar, and the results are given. The angle of torsion employed was always the same, equal to 60°; and the threads by which the hollow cylindrical magnet was suspended were the same during nearly the whole of the Survey, having been in use for upwards of three years.

The barometer was observed at every station. At the principal stations the standard was in use, and at the others the portable, from the greater trouble and risk attending the landing the large barometer. The curve of the barometer, corrected to 32°, is exceedingly regular, the time of maximum and minimum being identical for every station on the Survey, viz. 21 or 22 hours for the maximum, and 3 or 4 hours for the minimum; the oscillation at every station amounting to about one-tenth of an inch. The curve of the tension of vapour, as determined by the dry and wet bulb, is irregular, which is communicated to the barometric curve when applied to it; but this irregularity may be owing to the very short time the instruments were observed at some of the stations; and for the purpose of ascertaining whether this really was the case, I projected the curves of the barometer, corrected to 32° of temperature, at some of those stations where observations had been carried on for more than a month, and likewise, in addition, corrected for tension of vapour, thus giving the oscillation of the column of dry air. There is certainly not the same regularity perceptible as in the oscillation of the barometer, yet there is considerable uniformity in the oscillation of the pressure of the column of dry air, the maximum generally occurring at 19 or 20 hours, and the minimum at 2, 3 or 4 hours. The range is likewise considerably greater.

The tension of vapour was generally a minimum at 18 hours, when it amounted to 0·8 of an inch. It then increased to 2 hours, when it might amount to 0·9 of an inch, and then gradually diminished, having but one maximum and one minimum in the 24 hours. In the islands of the Archipelago the air is nearly saturated with moisture, and at Singapore the maximum difference between the dry and wet bulb at 1 or 2 P.M. amounted to not more than 7°, and then gradually and uniformly diminished till sunrise, when the difference was rarely more than a degree. There appears to be one maximum and one minimum of the dry and wet bulb thermometer, the former at noon or 1 P.M., the latter at 18 or 19 hours; the greatest and least differences between the two thermometers occurring, as has been observed, likewise at these hours.

In addition to the above, a standard thermometer by Newman was observed; one maximum and one minimum occur in the twenty-

four hours, viz. at 2 and at 18 hours. The oscillation varies considerably; the smallest oscillation being observable at Singapore, where the standard thermometer was placed inside the Observatory, and exposed to a current of air passing through the building. The range was greatest at Moulmein and in Sumatra; but at these observatories, the instruments being under canvas, the direct influence of the sun's rays was very great. In addition to these thermometrical observations, there was likewise in use a Solar Radiation Thermometer, the bulb of which was tinged of a dark purple colour, not absolutely black; this instrument was placed on a table outside the observing tent, and the bulb beyond the edge of the table exposed to the sun. This thermometer was read off from 19 hours to 4 or 5 hours P.M., and the maximum of the day, with the time at which it was observed, recorded. The minimum thermometer (self-registering) was placed at night, and in a similar manner, on the table outside the tent, and the minimum read off in the morning. These observations have been recorded in the Tables, but have not been deemed of sufficient importance to lay them down in curves. This completes that portion of the Survey which relates to the hourly changes of the magnetical and meteorological instruments.

The author next proceeds to the consideration of the absolute determinations, which formed the principal object of the Survey, viz. Latitude, Longitude, Dip, Horizontal Force and Declination. The method adopted in making these observations was as follows. On arrival at a station, the tent was pitched, and the instruments prepared. These were a six-inch dip circle, a portable declinometer, an altitude and azimuth instrument by Robinson, and a chronometer which had been long in use, and was scarcely trustworthy for a fixed rate. Commencing the following morning, the first instrument set up was the declinometer, and as the suspension-thread was thicker than necessary, to obviate the necessity of frequent renewal, the brass weight for removing the torsion was allowed to swing for a couple of hours. During this interval the observations for dip were completed, and by 9 A.M. the collimator magnet was in the box, the altitude and azimuth instrument in rear of it, and in adjustment with it. Sights were then taken with the sextant and artificial horizon for time, and with the altitude and azimuth instrument in connection with the collimator magnet for declination. From 10 A.M. to 11 A.M. observations of deflection at four different distances, and of vibration, were made with the portable declinometer, and the telescope of the altitude and azimuth instrument, for the absolute value of the horizontal force. At noon circum-meridional altitudes were observed for latitude, and in the afternoon equal altitudes were taken to confirm the observations of the morning. The instruments were then packed up and sent off to the next station. This was the system adopted whilst travelling; but at the fixed stations a great number of additional observations were made of horizontal force, dip and declination.

The method of grouping the results at the different stations for the purpose of drawing the isoclinal, isodynamic and isogonic lines,

is then described. For the isoclinal lines, four groups were formed: the first consisting of forty stations in Singapore, Borneo and Java; the second of thirty stations in Sumatra; the third of thirty stations at sea; and the fourth of thirty of the principal stations in the Archipelago.

The first gave for the latitude of its central station 6° 17′ south; longitude 108° 55′ east; and 27° 01′·5 south dip. These thirty stations form so many equations of condition, and were combined by the method of least squares, as practised by Colonel Sabine in his Magnetic Survey of the British Isles: from the final equations it was found that the dip increased 1′·940 for each geographical mile perpendicular to the isoclinal lines, and that the direction of the latter is from north 86° 06′ east to south 86° 06′ west.

In the Sumatran group the latitude of the central station was 0° 08′ south; longitude 100° 31′ east; dip 16° 36′·6 south; the rate of increase of dip being 2′·021 for each geographical mile perpendicular to the isoclinal line, the direction of which is from north 83° 28′ west to south 83° 28′ east.

In the third, or sea group, the latitude of the central station is 2° 38′; longitude 110° 05′ east; dip 9° 11′·7 south; the resulting equations give the isoclinal line running from north 87° 26′ west to south 87° 26′ east; and the dip in the line at right angles to it increases at the rate of 1′·993 for each geographical mile.

The last or general group of thirty of the principal stations in the Magnetic Survey give the resulting equations at a mean latitude of 0° 09° north, mean longitude 104° 44′ east, and mean dip 14° 40′·4 south; the isoclinal line running from north 86° 39′ west to south 86° 39′ east; and the dip in the line at right angles to it increasing at the rate of 1′·953 for each geographical mile.

Nearly the same method has been pursued in laying down the lines of equal horizontal force. In one group this line forms an angle of north 67° 48′ west to south 67° 48′ east with the meridian, and the line perpendicular to it proceeds at the rate of ·0008249 for each geographical mile, the central station being in latitude 0° 38′·5 north, and longitude 102° 26′ east. In another group at the central station 3° 40′ south, and 106° 33′ east, the line of equal horizontal force forms an angle of 60° 57′ with the meridian, and increases at the rate of ·0005855 for each geographical mile on the perpendicular line.

The lines of declination have been laid down with reference to the declination at each station individually, the particular form of the lines rendering the method of grouping inapplicable.

Total force.—The total force was not determined directly, but by the formula $f=h \cdot \sec \delta$, where h is the horizontal intensity, δ the dip, and f the total force; and as the dip changes rapidly, but regularly, whilst the horizontal intensity changes slowly, but irregularly, it is evident the lines of total force will follow in some degree the regularity of the intervals between the lines of dip.

The total force for each station was determined by the above formula, and equations of condition, combined by the method of

least squares, determined the direction of the isodynamic line, in the mean latitude of 3° 05′ south, longitude 106° 47′; where the mean total force was 8·745. The angle which the isodynamic line forms with the meridian is from north 85° 36′ west to south 85° 36′ east, and increases southerly at the rate of ·001073 for each geographical mile. The line of least intensity, in the space over which the Survey was carried, appears to agree almost exactly with the line of no dip.

To recapitulate briefly respecting the position of the principal lines. The line of no dip cuts the meridian at an angle approaching a right angle, and near the parallel of latitude of 8° north in the longitude of Singapore; the rate of progression being almost exactly two miles of dip to one mile of latitude. The line of least total force varies but little from the line of no dip, forming indeed a somewhat larger angle with the meridian. The direction of the maximum horizontal force forms an angle of nearly 65° with the meridian from north-west to south-east, but neither the maximum horizontal nor the minimum total intensity appear to be, strictly speaking, lines. In taking two stations having the same value, the one to the north and the other to the south of the position of greatest horizontal intensity, and dividing the distance between them, it might be supposed that this would be the position of the line of greatest horizontal intensity; but the maximum horizontal intensity appears not to be a line, but a space or belt extending for a considerable distance on either side of this line; the value in this space being 8·20. The minimum total force was 8·149; the station being Madras, in latitude 13° 04′ north, longitude 80° 16′ east, dip 7° 34′ north, horizontal intensity 8·078, and declination 0° 56′ 09″ east.

In addition to the above, observations were made at sea, consisting of temperatures of the air and sea; the dry and wet bulb, the standard thermometer, and, whenever practicable, of the dip with a Fox's Dip Circle. The author remarks that it is impossible to overestimate the practical value of this instrument at sea in low latitudes. The dip changes two minutes for every mile of latitude. The seas are generally smooth. When astronomical observations cannot be taken, the dip circle becomes a truly valuable latitude instrument, and in a few minutes, by day or by night, the latitude of the ship can be correctly ascertained.

The author concludes by remarking that he has found Colonel Sabine's many works on magnetism of considerable use, more especially 'Lloyd and Sabine's Magnetic Survey of Great Britain and Ireland.'

January 23, 1851.

In consequence of the decease of the Marquis of Northampton, who filled the office of President for eleven years, the Society did not meet on this day.

January 30, 1851.

SIR RODERICK IMPEY MURCHISON, V.P., in the Chair.

The following papers were read:—

1. "On the Oxidation of Ammonia in the Human Body, with some remarks on Nitrification." By Henry Bence Jones, M.D., F.R.S. &c. Received December 18, 1850.

The author having shown, in a paper lately communicated to the Royal Society, that the effect of tartrate of ammonia on the acidity of the urine was totally different from that of tartrate of potash, and that carbonate of ammonia, taken in very large quantities, did not produce any alkaline reaction of the urine, but that, on the contrary, the acidity was rather increased than diminished by such doses, repeated the experiments with carbonate of ammonia, hoping to obtain more decided results. Although, from these experiments, it was again apparent that no diminution of the acid reaction resulted from taking carbonate of ammonia, yet the fact of any great increase in the acidity of the urine could not be determined. In his former paper, the author suggested that an inquiry into the occurrence of nitric acid in the urine would probably give the solution of this unexpected effect of carbonate of ammonia: and he was led to undertake the experiments described in the present paper with the view of detecting the presence of that acid under particular circumstances.

The indigo test for nitric acid being more delicate than the protosulphate of iron test, it was chiefly employed; but a mixture of starch with a drop or two of hydriodate of potash and hydrochloric acid was found to be a far more delicate test than either. Beginning with 10 grs. of nitrate of potash added to 10 oz. of urine, it was found at last that as little as 1 gr. of nitre to 10 oz. of urine could be detected with the greatest certainty and clearness when the starch test was used; but this quantity could not be detected as surely by the indigo test.

Experiments are described in which carbonate of ammonia was given, in doses varying from 40 grs. to 7 grs., to a healthy man in whose urine no nitric acid could previously be detected; and the urine was tested at intervals of several hours after each dose. From these it appears that 10 grs. was the smallest quantity that gave decided evidence of nitric acid by both tests.

Having satisfied himself that when carbonate of ammonia was taken small quantities of nitric acid passed off in the urine, the author made similar experiments with tartrate of ammonia, administered in doses of 60 and 40 grs.; and in each case the starch test gave evidence of the presence of nitric acid in the urine some hours after. Similar experiments with the muriate of ammonia are next described; and in these the presence of nitric acid in the urine was readily detected three hours after the administration of the dose, even when it was so small as 10 grs.

From an experiment described in the paper, it was shown, that by

a simple combustion of ammonia out of the body, as well as in the body, nitric acid was produced. From other experiments it appears that urea, also, by oxidation, whether in the body or out of the body, gives rise to nitric acid.

Having found that nitric acid was produced more readily and frequently than had been supposed to be the case, the author was led to try whether combustions in the atmosphere without ammonia could not give nitric acid. The presence of this acid was, in consequence, detected in the products of the combustion of alcohol, of coal, of a wax candle, and of hydrogen.

As this led to the supposition that nitric acid might exist in rain-water at all times, experiments were made on the rain-water collected on wet days in London, and the presence of nitric acid was discovered by the starch and also by the indigo test.

The conclusions the author comes to from his experiments are:—

1. That the action of oxygen takes place in the body, not only on hydrogen, carbon, sulphur and phosphorus, but also on nitrogen.

2. That in all cases of combustion, out of the body and in the body, if ammonia be present, it will be converted partly into nitric acid.

3. That the nitrogen of the air is not indifferent in ordinary cases of combustion, but that it gives rise to minute quantities of nitric acid.

He further remarks, that the production of nitric acid from ammonia in the body adds another to the many instances of the action of oxygen in man; and that the detection of nitric acid in the urine may lead to the conclusion, that the blood is being freed from ammonia, or from substances closely related to it, as urea, or possibly caffein and other alkaloids.

2. "Description of a Muscle of the striped variety, situated at the posterior part of the choroid coat of the Eye in Mammals, with an explanation of its mode of action in adapting the Eye to distinct vision at different distances." By George Rainey, Esq., M.R.C.S. Communicated by Joseph H. Green, Esq., F.R.S. Received December 24, 1850.

Respecting this muscle, the author observes that it occupies about the posterior two-thirds of the choroid coat, its fibres lying in different planes, the most superficial being immediately beneath the membrana pigmenti, the deepest extending almost as far as the vasa vorticosa; that these fibres pass in different directions, some going from before to behind, and others intersecting these at various angles: altogether they receive the pigment membrane, the retina and the vitreous humour as into a cup.

From the connection of these fibres with the choroid coat, the author calls them the choroid muscle. He has not been able to trace them nearer to the ciliary ligament than about two-thirds of the distance from the centre of the choroid to the border of the cornea, in which situation the fasciculi become broken up, and gradually degenerate into filamentous tissue. No striped fibres can be detected in the so-called ciliary muscle or ligament. These the author

considers to be merely for the purpose of connecting the sclerotic and choroid anteriorly, as these tunics are also connected by cellular tissue posteriorly, but very loosely in their middle.

The author next describes the manner in which these fibres may be most easily displayed, and, as the sheep's eye answers best for this purpose, his description is given in reference to it. The posterior two-thirds of the eye of the sheep being turned inside out, and all the retina washed away, a very thin portion of the tapetum is raised and floated upon a glass slide, and a thin glass cover is then placed upon it, without making the least pressure, which would, by crushing the very soft primary fasciculi, render the transverse striæ irregular and confused. If in this state it be examined by the microscope, the muscular fibres, in consequence of their transparency, and being covered by the membrana pigmenti, and some of the iridescent fibres of the tapetum, will be very indistinct and scarcely distinguishable; but if a small portion of acetic acid, or some aqueous solution of chlorine, be brought in contact with it, the membrana pigmenti, and especially the iridescent fibres, will instantly shrink, and, becoming transparent, disappear, and the fasciculi of the choroid muscle will come into view. Should the acetic acid have been too strong, the transverse marking will be very faint and scarcely discernible; but if the acid be washed away with water, or, what is better, a weak solution of any saline substance, such as chloride of potassium or sodium, iodide of potassium, &c., the transverse marking will become very distinct; should the solution be strong, the colour of the iridescent fibres will be reproduced. In the eye of those animals whose tapetum is scaly, as in the Cat, or where the whole of the choroid is lined by black pigment, as in the human subject, the acetic acid is of no use, and the muscle is displayed with much greater difficulty; the author, however, states that he has succeeded in displaying this muscle in the choroid of the Horse, the Cat, and in that of the human eye*.

The author next considers the action of the choroid muscle. This, from the manner in which the muscle embraces the vitreous humour, is, in his opinion, to compress this humour and carry the lens forwards. But this, he observes, considering how the capsule of the lens and the ciliary processes fill up the posterior chamber of the aqueous humour, cannot be effected without displacing some of the fluid in that chamber; and he concludes that the aqueous humour in the posterior chamber being pressed by the capsule of the lens (forced forwards by the action of the choroid muscle) against the ciliary processes, forces the blood out of their vessels into the choroid veins, and thus enlarges this chamber at its circumference, whilst it diminishes it from behind forwards, or, in other words, moves the lens forwards. The author then observes, that, as the effect of the action of the choroid muscle is to separate the choroid coat from the sclerotic, all pressure is taken off the choroid veins at the time it is made upon the ciliary processes, and therefore that

* The author has found, since the paper was read, that a solution of citric acid, one drachm of acid to the ounce of water, is the best substance for rendering the iridescent fibre transparent.

every facility possible is afforded for the emptying of the vessels of these processes. He hence infers that the office of these processes is to allow of the displacement of fluid when the lens is carried forwards; and when the choroid muscle ceases to act, by the re-distension of their capillaries, to carry the lens back into its place. The author then enters into a mathematical examination of the data furnished by these facts, to show how exactly they fulfill the conditions necessary for adapting the eye, viewed as an optical instrument, to distinct vision at different distances. The author considers that there is no analogy either in structure or function between those fibres in mammals occupying a situation similar to that occupied by a true muscle in Birds (the ciliary muscle), but that the true analogue of the ciliary muscle in birds is the choroid muscle in mammals, the chief difference between them being in situation. In the Pigeon, he finds that the ciliary muscle is inserted into the choroid coat, along which it can be traced as far back as about $\frac{1}{8}$th of an inch behind the iris, so that its action would be to draw the choroid tense upon the vitreous humour, and thus to compress it similarly to the choroid muscle in the mammal. Besides, in the Bird he finds no muscular fibres at the posterior part of the choroid. The ciliary muscle is more distinct, and appears to be stronger than the choroid, but this the author attributes to the fibres of the one being much more collected, and therefore limited to a much smaller space than those of the other.

February 6, 1851.

GEORGE RENNIE, Esq., V.P., in the Chair.

A paper was read, entitled "On the Supply of Water from the Chalk Stratum in the neighbourhood of London." By John Dickinson, Esq., F.R.S. Received January 6, 1851.

The object of this paper is to explain and illustrate the supply of subterranean water which is always found at certain depths in the chalk strata; the circumstances that influence its natural outflow by springs and rivers; the practicability of draining off that water by an artificial mode of exhaustion; and the changes that would be produced by carrying such an operation into effect on a large scale.

It is stated, that numerous perennial streams issue from the elevated ridges of the chalk strata, those in Kent and Surrey flowing from south to north, and those in Buckinghamshire, Herts and Essex, flowing from north to south; and that in each case the dip of the strata corresponds with the fall of the country and the direction of the streams. These rivers are considered to be the natural outflow of the rain-water imbibed by the chalk, the accumulation of which, as explained by Dr. Buckland, is in a subterranean reservoir; and according to the periodical filling and exhaustion of this, the springs and streams alternately decrease and are augmented. The circumstances regulating this change, which have been observed and

experimented upon by the author during the last forty years, are explained in the paper. In the year 1835, he adopted a method of ascertaining the supply of subterranean water, by the use of a very simple but effective instrument, contrived and used by the late Dr. Dalton for that purpose; and he has supplied a series of monthly observations, extending over fourteen years, both of the rain falling on the surface, and of that which is found to percolate through to a lower level for the supply of springs and rivers.

From his observations—

	inches.
The annual average of rain in the north-western part of Herts is	25·92
The average fall of rain in the first six months of the year is	11·12
The average from July to December inclusive	14·80
The average in six months, from April to September inclusive	12·17
The average percolation through the Dalton gauge, from April to September inclusive, is	0·62
And from October to the following March inclusive	9·61
Total average annual amount of percolation	10·23

The rivers and springs supplied from the chalk are generally found to be in fullest flow about June, and to be most reduced in December; and the cause of this variation is considered to be the time that the descending rain requires to percolate through the crevices and fissures of the lofty chalk hills, and to spread laterally in the reservoirs till it reaches the outlet springs.

The variation in the quantity of rain is found to range from 21·10 inches to 32·10 inches. The variation in the amount of percolation is found to be far greater, viz. from 3·10 inches to 19·28 inches. Consequently there is a variation in the springs, and in the flow of the rivers, much greater than in proportion to the fall of rain. The author shows that the season of the rain falling is the main incident in the supply of these perennial springs; and that their outflow is proportional to the percolation which takes place during winter into the lower beds, the summer rains being evaporated or taken up by vegetation.

He gives it as his opinion, that it is possible to drain off, by artificial means, great part of any river flowing out of the chalk; such rivers being truly the natural drain and outlet of the subterranean reservoir therein. He shows, by precise measurements and carefully recorded observations, that the subterranean water has a movement, with a declivity of 13 feet 6 inches to the mile, in the direction of the dip of the strata and of the fall of the streams; and he states that the crevices or water-channels in the chalk are larger in the neighbourhood of a stream. He therefore assumes that if a large and deep well were sunk in any such locality, and the water in it, by being pumped off by steam-power, were brought down to and kept at a lower level, a deep-seated artificial vent being thus formed, the water would be so drained off from the reservoir that the springs would be dried up, and the river be partially or entirely deprived of its flow of water.

February 13, 1851.

SIR BENJAMIN C. BRODIE, Bart., V.P., in the Chair.

A paper was read, entitled, "On Rubian and its Products of Decomposition." By Edward Schunck, Esq., F.R.S. Received January 9, 1851.

After adverting to the obscurity in which the inquiry concerning the state in which the colouring matter of Madder originally exists in this root is involved, the author refers to the change which takes place in the root, especially if in a state of powder, during the lapse of time, and to the little light which has been thrown by chemical investigations on the nature of the process by which the change is effected. He states that it has been suspected by several chemists that there exists originally some substance in madder, which, by the action of fermentation, or oxidation, is decomposed, and gives rise by its decomposition to the various substances endowed either with a red or yellow colour, which have been discovered during the chemical investigations of this root. In his papers on the colouring matter of madder, he has described four substances derived from madder, only one of which is a true colouring matter, but all of them capable under certain circumstances, as for instance in combination with alkalies, of developing red or purple colours of various intensity. After referring to the opinions of M. Persoz and the investigations of Mr. Higgin relative to xanthine and alizarine, the author observes that, by adding a variety of substances to an extract of madder with cold water, he was enabled to ascertain under what circumstances and by what means the tinctorial power of the liquid is destroyed, and consequently what is the general character of the substance or substances to which it is due. He found that by adding sulphuric, or muriatic acid to the extract, and heating, the liquid, after neutralization of the acid, was no longer capable of dyeing. The tinctorial power was also destroyed by the addition of hydrate of alumina, magnesia, protoxide of tin, and various metallic oxides, but not by carbonate of lime, or carbonate of lead. In all cases in which the property of dyeing in the extract was destroyed, he invariably found that its bitter taste and bright yellow colour were destroyed. Having shown, in his former papers on this subject, that the intensely bitter taste of madder and its extracts is due to a peculiar substance to which he has given the name of *rubian*; and as it appeared from these preliminary experiments that this substance, though itself no colouring matter, is in some way concerned in the changes whereby a formation of colouring matter is induced in aqueous extracts of madder, he proposed to himself to examine its properties and products of decomposition more in detail than he had hitherto done.

After numerous experiments, undertaken with the view of obtaining pure rubian in quantities sufficiently large for the purposes of examination, he discovered a property of that substance, by which he was enabled to obtain it in a state of purity, namely the remark-

able attraction manifested by it towards all substances of a porous or finely-divided nature, which is perhaps more characteristic of it than any other. The method he finally adopted, and which, he states, surpasses all others in facility and certainty of execution, is fully detailed.

Rubian, when prepared according to this method, is a hard, dry, brittle, shining, perfectly uncrystalline substance, similar to gum or dried varnish. It is not in the least deliquescent, as xanthine is described to be. In thin layers it is perfectly transparent, and of a beautiful dark yellow colour; in large masses it appears dark brown. It is very soluble in water and in alcohol, more so in the former than in the latter; but insoluble in æther, which precipitates it in brown drops from its alcoholic solution. Its solutions have an intensely bitter taste. When it is pure, its solution in water gives no precipitates with the mineral or organic acids, nor with salts of the alkalies or alkaline earths. Basic acetate of lead, however, gives a copious light red precipitate in a solution of pure rubian, the solution becoming colourless; but this is the only definite compound of rubian with which the author is acquainted. He states that rubian cannot be considered as a colouring matter in the ordinary sense of the word. It is decomposed by acids, alkalies, chlorine, heat and ferments. The formula for rubian appears to be $C^{56} H^{34} O^{30}$.

The action of sulphuric and of muriatic acid on rubian is stated to be precisely the same. The products of the decomposition by those acids are,—1st, *Alizarine*; 2nd, the substance which the author formerly termed Beta-resin, but which he now calls *Verantine*; 3rd, the substance which the author in former papers has called Alpha-resin, but to which he now prefers giving the name of *Rubiretine*; 4th, a body which has not hitherto been observed, and which he denominates *Rubianine*; and 5th, a sugar obtained from the acid liquid after the complete decomposition of the rubian.

1. The alizarine obtained from the decomposition of rubian exhibits all the usual properties of this well-known substance. Its colour is dark yellow, without any tinge of brown or red. The crystals possess a lustre which the author has never seen equalled in this substance. The analysis gave the formula $C^{14} H^5 O^4$. Thus by simply losing 14 equivs. of water, 1 equiv. of rubian is converted into 4 equivs. of alizarine.

2. Verantine, in most of its properties, coincides with the substance to which the author formerly gave the name of Beta-resin of madder. When prepared according to the method described in the paper, it is obtained in the form of a reddish-brown powder. When heated on platinum foil, it melts, and then burns away without leaving any residue. It dissolves in concentrated sulphuric acid, with a brown colour, and is reprecipitated by water in brown flocks. On heating the solution in concentrated sulphuric acid, it becomes black, sulphurous acid is disengaged, and the substance is decomposed. Concentrated nitric acid dissolves it on boiling, with a disengagement of nitrous acid, forming a yellow liquid, from which nothing separates on cooling. It is almost insoluble in boiling water, but readily

soluble in boiling alcohol, and is again deposited, on the alcohol cooling, as a brown powder, which is its most characteristic property.

From an experiment described in the paper, the author is inclined to conclude that alizarine and verantine are capable of forming a double compound with alumina soluble in boiling water, and that a mixture of the two in the proportion in which they exist in this compound constitutes what has been called purpurine.

Although the difficulty of obtaining pure verantine in sufficient quantity for the purposes of analysis prevented the author from determining its composition with the requisite accuracy, he obtained approximations sufficiently near to remove almost all doubts on the question. The formula deduced from four experiments is $C^{14} H^5 O^5$; so that it appears that verantine differs from alizarine by containing 1 equiv. more of oxygen.

3. Rubiretine is identical with the substance which the author formerly called Alpha-resin of madder. He endeavoured in vain to determine the atomic weight of this substance; but states that there is only one formula which is in accordance with his analyses, and at the same time satisfactorily explains its formation. This formula is $C^{14} H^6 O^4$; so that 2 equivs. of verantine and 2 equivs. of rubiretine with 12 equivs. of water give 1 equiv. of rubian.

4. Rubianine greatly resembles rubiacine in its appearance and many of its properties. It may however easily be distinguished by several characteristics, and above all by its composition. It is obtained from a solution in boiling alcohol in the form of bright lemon-yellow silky needles, which when dry form an interwoven mass. It is less soluble in alcohol, but more so in boiling water, than any of the preceding substances. On the solution in boiling water cooling, it crystallizes out again in yellow silky needles. Its colour is lighter than that of rubiacine. When heated on platinum foil, it melts to a brown liquid; then burns, leaving a carbonaceous residue, which on further heating disappears entirely. It is soluble in concentrated sulphuric acid, with a yellow colour; the solution on boiling becomes black, and gives off sulphurous acid. It is not affected either by dilute or concentrated nitric acid even on boiling; it merely dissolves in them, and, on the acid cooling, crystallizes out again as from boiling water.

There are three formulæ, all of which give for 100 parts of this substance numbers not widely differing from those deduced from the analyses, viz. $C^{28} H^{17} O^{13}$, $C^{32} H^{19} O^{15}$, $C^{44} H^{24} O^{20}$, but the last is that with which they best agree.

5. The sugar is always obtained in the form of a transparent yellow syrup, which neither crystallizes, however long its solution may be left to stand, nor becomes dry unless heated to 100° C. There are two formulæ, $C^{14} H^{14} O^{14}$ and $C^{12} H^{12} O^{12}$, both of which agree with the analyses of this substance, and explain its formation. The author states the views to which each of these formulæ gives rise, and the reasons for adopting the latter.

Although five substances are produced by the action of acids on rubian, the author does not consider that these substances are all

formed together, or in other words, that one atom of rubian, by its decomposition, gives rise to all five at the same time; but that, from the composition of these substances as compared with that of rubian, it follows, that the decomposition affects three separate atoms of rubian. One of these atoms loses 14 atoms of water, and is converted into alizarine. The second loses 12 atoms of water, and then splits up into verantine and rubiretine. The third takes up the elements of water, and then splits up into rubianine and sugar. Whether it would be possible to confine the decomposition of rubian entirely to one of these processes, or whether all three are essential, he considers is a question of the highest importance, not so much in a theoretical as in a practical point of view; and that should any chemist succeed in changing rubian entirely into alizarine, he would be the means of giving a great stimulus to many branches of manufacture and adding largely to the national wealth.

February 20, 1851.

LIEUT.-COL. SABINE, V.P. and Treas., in the Chair.

The Chairman stated, that Mr. John Scott Russell, who ceased at the last Anniversary to be a Fellow of the Society in consequence of the non-payment of his subscription, had applied to the Council to be reinstated, alleging that his numerous avocations and absence from England caused him to overlook the fact of his subscription not having been paid. The Chairman therefore gave notice, that, in accordance with the statutes, the question of the readmission of Mr. Russell into the Society would be put to the vote at the ensuing meeting.

A paper was in part read, entitled "On Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances," by Lieut.-Col. Sabine, V.P. and Treas. R.S., &c.

February 27, 1851.

SIR FREDERICK POLLOCK, Lord Chief Baron, V.P., in the Chair.

The question of Mr. J. S. Russell's re-admission into the Society was put to the vote and carried.

Lieut.-Colonel Sabine's paper, entitled "On Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances," was concluded.

In a discussion of the *two-hourly* observations of the magnetic declination, made in 1841 and 1842 at the observatories of Toronto and Hobarton, published in 1843 and 1845, the author expressed an opinion that the magnetic disturbances, of large amount and apparently irregular occurrence, commonly called magnetic storms or shocks,

would be found, when studied in their mean effects on the magnetic direction and force extending over a sufficient period of time, to be subject to *periodical laws*, connecting them with the seasons of the year and the hours of the day at the particular stations.

In preparing for the press the *hourly* observations of the declination in the years 1843, 1844 and 1845, at the same two stations, the author found his previous opinions strongly confirmed; and believing that the evidence thus obtained of periodical laws is far too systematic, and rests on a series of too long duration to make it probable that it will be otherwise than confirmed by the continuation of the observations in subsequent years, he has been induced to make it the subject of a communication to the Royal Society; although it is probable that the exact periods, and the mean numerical values of the effects produced, or their proportions to each other in the different seasons and at the different hours, may hereafter receive modifications.

The disturbances which are the subject of this investigation have two leading characteristics,—1st, the irregularity of their occurrence, many days together frequently passing without any trace of them being discoverable; and 2nd, the large amount of deviation from a mean or normal position, to which the needle is subject during their continuance. It is this last feature which has led to their general recognition, and to the establishment of the fact, that when they take place their influence usually extends simultaneously, or nearly so, over all parts of the globe at which observations have been made. The same feature still affords their best and most convenient distinguishing mark.

Availing himself of this characteristic, the author separated from the whole mass of hourly observations in the three years at both stations a sufficient portion of disturbed observations to form an adequate basis for investigation. The portion thus separated consisted at Toronto of the 1650 largest deviations of the declination magnet from its mean position; the whole number of hourly observations in the same period being 22,376, and the proportion therefore being 1 in 13·6; and at Hobarton of the 1479 largest deviations, the whole number being 21,436, and the proportion 1 in 14·5. Of the 1650 disturbed observations at Toronto, 472 were in 1843, 612 in 1844, and 566 in 1845. Of the 1479 disturbed observations at Hobarton, 415 were in 1843, 562 in 1844, and 502 in 1845. Whence it appears, that at both stations, situated as they are in opposite hemispheres and nearly at opposite points of the globe, 1843 was the least disturbed year of the three, and 1844 the most so. Taking the number in 1845 as unity, the numerical proportions at each station are as follows:—

	Toronto.	Hobarton.
1843........	0·84	0·83
1844........	1·08	1·12
1845........	1·00	1·00

This accordance, and the fact that the separated disturbed observations in these years occurred for the most part on the same days at

the two stations, are noticed as corroborating the conclusions, derived from former investigations, of the character of these disturbances as affecting contemporaneously the most distant parts of the globe.

Several tables are given containing the distribution of the separated observations,—1st, into the several *months*, and 2nd, into the several *hours*, of their occurrence, the hours being those of local time. This is done, first, in regard to the *number* of disturbed observations in the several months and hours; a separate account being taken of those which are easterly, and of those which are westerly deflections; and second, in regard to the *mean numerical values* of the deflections at the different hours, distinguishing easterly and westerly, and taken on a daily average throughout the year. The results of this analysis are carefully stated; and are conclusive, in the author's opinion, in manifesting the existence of periodical laws in the times of occurrence and in the effects of the larger disturbances; these laws having points of remarkable analogy at both stations, and showing a causal connexion to subsist between the disturbances on the one hand, and the seasons of the year and hours of the day on the other.

The author remarks on the practical bearing which the establishment of such laws must have on the researches, which have been recently brought before the Royal Society by one of its most distinguished members, into the physical causes of the periodical variations of terrestrial magnetism, particularly of the *diurnal* magnetic variation. The present investigation shows that the latter phenomenon must now be regarded as consisting of *two* periodical variations, superimposed upon each other, having extremely dissimilar laws, and probably therefore different immediate causes. These constituent parts of the variation will bear different proportions to each other in different parts of the globe, and in many parts of the globe it will be necessary, in the author's opinion, to separate the whole diurnal variation into its constituents in order to study their respective physical causes. At Toronto and Hobarton the diurnal variation occasioned by the disturbances forms a clearly recognizable part of the whole diurnal variation; the greater part, if not the whole of that remarkable phenomenon, which Mr. Faraday has termed the "nocturnal episode," appearing to be attributable to this cause.

The author concludes by remarking, that the investigation which forms the subject of this paper cannot be regarded as complete, until the influence of the larger disturbances on the phenomena of the magnetic *inclination* and *force* have undergone a similar examination. This he hopes to have a future opportunity of submitting to the Society.

March 6, 1851.

GEORGE RENNIE, Esq., V.P., in the Chair.

In accordance with the statutes, the following List of Candidates for admission into the Society was read by the Secretary:—

Charles Cardale Babington, M.A.
Thomas Snow Beck, M.D.
Daniel Blair, M.D.
Alexander Bryson, M.D.
Charles James Fox Bunbury, Esq.
Rev. Jonathan Cape, M.A.
Rev. John Cumming, D.D.
Hewitt Davis, Esq.
George T. Doo, Esq.
Edward B. Eastwick, Esq.
Charles M. Elliot, Capt. Madras Engineers.
Robert Fitzroy, Capt. R.N.
Henry Gray, Esq.
Wyndham Harding, Esq.
John Hawkshaw, Esq.
John Higginbottom, Esq.
John Russell Hind, Esq.
Augustus Wm. Hofmann, Ph.D.
Thomas Henry Huxley, Esq.
Edward Augustus Inglefield, Com. R.N.
Richard Hartley Kennedy, Esq.
William Edmund Logan, Esq.
Edward Joseph Lowe, Esq.
Charles Manby, Esq.
Joseph Maudslay, Esq.
James Paget, Esq.
Hugh Lee Pattinson, Esq.
Apsley Pellatt, Esq.
Rev. Bartholomew Price, M.A
Lovell Augustus Reeve, Esq.
Julius Roberts, Lieut. R.M.A.
George Gabriel Stokes, Esq.
William Thomson, Esq.
Augustus Waller, M.D.
Nathaniel Bagshaw Ward, Esq.
Arthur Parry Eardley Wilmot, Com. R.N.
Forbes Benignus Winslow, M.D.
Charles Younghusband, Capt. R.A.

The following papers were read:—

1. "On the Explanation of the so-called 'Mysterious Circles.'" By the Rev. Robert H. Atherton. Communicated by the Earl of Rosse, P.R.S. &c. Received November 28, 1850.

The author refers to explanations which have been given of the phenomenon, of which he proposes to give a new one, and points out various courses which the propelled air may be supposed to to take with reference to the cards. He then offers what he considers to be the true explanation of the phenomenon. He considers that no sooner has the air struck upon the loose card, than it is reflected, spreading partly, if not entirely, over its surface, and then ascends, carrying with it the interposing atmosphere, and excluding the surrounding air. This, he submits, is analogous to ordinary suction, and by this means the loose card is at once drawn up and fixed. He considers that the reflected air, thus rising and driven out by the descending current, will have additional power when the fixed card is considerably larger than the other.

2. "On the relation of the Direction of the Wind to the Age of the Moon, as inferred from observations at the Royal Observatory,

Greenwich, from 1840 November to 1847 December." By G. B. Airy, Esq., F.R.S. &c., Astronomer Royal. Received January 9, 1851.

The author states, that in a voyage to Shetland, in the year 1849, he heard allusions to the belief entertained generally by Norwegian seamen, that a northerly wind may always be expected about the time of new moon. The expression of this belief was so positive, and the implication of the interests of the persons entertaining it was so distinct, that it appeared to him extremely probable that there was some physical foundation for it. At the first convenient opportunity he therefore took measures for discussing, with reference to this question, the directions of the wind at the Royal Observatory, during a period of rather more than seven years, as ascertained from the records of Osler's self-registering anemometer. He extended the research so far as to enable any one to judge whether there is any probable relation between any direction of wind and any age of the moon.

The collection and summation of the numbers was effected under the immediate superintendence of Mr. Glaisher; and great pains were taken to establish such checks on the operation that error is considered to be almost impossible.

The general result is contained in a table subjoined to the paper. This exhibits the number of hours during which the wind blew in each of sixteen equal divisions of the azimuthal circle, and also the number of hours of sensible calm, in the period extending (with very small interruptions) from 1840 November to 1847 December, arranged in reference to the days of the moon's age. The author remarks, that while this table shows that there is great uncertainty in the verification of an empirical law, even from nearly ninety lunations, it seems very distinctly to negative the asserted law which gave rise to the inquiry.

March 13, 1851.

Lieut.-Col. SABINE, R.A., V.P. and Treasurer, in the Chair.

The following papers were read:—

1. "On the Meteorology of the Lake District, including the results of experiments on the fall of Rain at various heights up to 3166 feet above the sea-level. Fourth paper. For the year 1850." By John Fletcher Miller, Esq., F.R.S., F.R.A.S. &c. Received February 21, 1851.

The observations detailed in this paper are similar to those described by the author in his former papers, and the results deduced from them agree generally with those previously obtained.

In the introductory remarks, the author states that he determined the heights of the different rain-gauges above the level of the sea by

means of an excellent Aneroid barometer (previously compared with a standard), and a standard barometer read simultaneously, or nearly so, at the sea-level; and he gives the heights of the several stations as thus estimated.

The most important fact connected with these observations is stated to be the discovery of a mountain station which promises to yield nearly one-third more rain than the hamlet of Seathwaite in Borrowdale, hitherto, with good reason, considered to be the wettest spot in the three kingdoms. This, the new station, "the Stye," on Sprinkling Fell, is about a mile and a half distant from Seathwaite, in a south-westerly direction, and 580 feet above it, at the extreme southern termination of the valley. The actual quantity of water measured in eleven months of 1850 was 174·33 inches; but as the receiver was found running over on four different occasions, the loss is calculated at 5 or 6 inches at least; and 5·67 is added, making the quantity in eleven months 180·00 inches. Adding to this 9·49 inches, the depth for January computed from that for January at Seathwaite, it appears that the whole depth of rain fallen at "the Stye" in 1850 was 189·49 inches. The author further remarks, that the wettest year since the commencement of the observations was 1848, when 160·89 inches fell at Seathwaite; and computing the fall at the new station for that year, we have 211·62 inches for the depth of rain at "the Stye" in 1848.

2. "On the Rolling Motion of a Cylinder." By the Rev. H. Moseley, M.A., F.R.S. &c. Received March 6, 1851.

The time occupied by a heterogeneous cylinder in oscillating upon a horizontal plane through a *small arc* has been investigated by Euler; and he has determined the *pressure* of the cylinder upon the plane when oscillating through *any* arc, applying the formula he has arrived at to find the pressure upon the plane at the highest and lowest points of oscillation. It is the object of the present paper to endeavour to extend this investigation to the *continuous rolling* of the cylinder, under which more general form its *oscillation* is obviously included as a particular case. In the first part of the paper, the time of rolling through any angle, and therefore of completing any given number of revolutions, is investigated; and in the second, the conditions of the pressure upon the plane at any period of a revolution. The complete determination of the time of rolling involves the integration of a function of the form $\int\left(\frac{\cos\theta-\alpha}{\beta-\cos\theta}\right)^{\frac{1}{2}}d\theta$, which is shown to be reducible to an elliptic function of the third order, capable of being expressed (by a theorem of Legendre) in terms of elliptic functions of the first and second orders, and therefore of having its numerical value calculated from the tables of Legendre. The theorem resulting from this reduction, when applied to the particular case of the *oscillation* of the cylinder, gives an expression for the time of oscillation, through *any arc*, of a pendulum having a cylindrical axis. If the diameter of this axis be assumed infinitely small, the case becomes that of a pendulum oscillating on

knife-edges; and the time of oscillation is expressed by the simple formula

$$t=\frac{2}{\pi}\,\mathrm{F}\left(c\,\frac{\pi}{2}\right),$$

where $\mathrm{F}\left(c\,\frac{\pi}{2}\right)$ represents that complete elliptic function of the first order whose modulus c is the sine of half the angle of oscillation. From this formula the times of oscillation through every two degrees of a complete revolution have been calculated in respect to a pendulum which beats seconds when oscillating through *small arcs*, and are given in the form of a table.

In the second part of the paper, general expressions are arrived at for the vertical and horizontal pressure of the cylinder upon the plane on which it rolls, at any period of a revolution; and these are applied to determine the conditions under which it will *jump*, or *slip* upon the plane. A *jump* will take place when the expression for the vertical pressure assumes a negative value; and whether such a jump will or will not take place in any revolution is determined by ascertaining whether the minimum value of the pressure in respect to that revolution be negative or not. The cylinder will *slip* if its friction on the plane fall short of the horizontal resistance X, determined as the necessary condition of its rolling. As the friction is measured by the product of the coefficient of friction by the vertical pressure V, it follows, that slipping will take place if $\frac{\mathrm{X}}{\mathrm{Y}}$ exceed the coefficient of friction; and whether it will or will not take place in any revolution is determined by ascertaining whether the maximum value of $\frac{\mathrm{X}}{\mathrm{Y}}$ in that revolution be or be not greater than the coefficient of friction. All these circumstances are investigated on the supposition that the centre of gravity of the cylinder is situated at any given distance from its axis, and that it is projected in any position with a given angular velocity, which angular velocity must be assumed $=0$, to get the case of an oscillatory cylinder. The investigation determines in this case the circumstances under which a pendulum oscillating by a cylindrical axis, or by knife-edges on horizontal planes, will jump or slip upon its bearings unless otherwise retained. If a finite value be assumed for the angular velocity sufficient to cause complete revolutions to be made, and if the diameter of the axis be assumed $=0$, the case will be arrived at of the pressure upon its bearings of a falsely-balanced wheel, or any unsymmetrical body revolving about a fixed horizontal axis, friction being neglected.

If the angular velocity of projection be supposed to be that obtained by the cylinder when its centre of gravity is at its highest point, the general formula for the vertical pressure assumes a simple form, under which it is readily applicable to the case of the falsely-balanced carriage wheel, a case which assumes a practical importance, from the fact that the driving wheels of locomotive engines are all, by reason of their cranked axles, falsely balanced unless counter-

weights be applied. The danger which might arise from this fact does not appear to have been at one time duly estimated; and when smaller engines were used than at present, and the axles were differently cranked, the author thinks there is reason to believe that the accidents which not unfrequently occurred with these engines (some of them attended by fatal results) were due to this cause. The fact seems first to have been brought prominently under the notice of engineers by the experiments of Mr. George Heaton of Birmingham, who caused a falsely-balanced wheel to roll round the periphery of a circular table, by means of an axis fixed to a pivot in its centre, and thereby exhibited the tendency to jump created by even a small displacement of the centre of gravity.

The analytical investigation in this paper shows how carefully the crank should be counterbalanced to provide the requisite security against the jumping of the wheel. It appears, that, assuming the weight of an engine to be from 20 to 25 tons, and of a pair of six-feet driving wheels from $2\frac{1}{2}$ to 3 tons, a displacement of the centre of gravity of the wheel of about 3 inches from its centre would be sufficient to cause it to jump at any instant when it attained a speed of sixty miles an hour.

A table is given in the paper of the displacements of the centre of gravity necessary to produce jumps at different speeds. These vary inversely as the squares of the speeds.

Before a jump can take place, there must be a slip of the wheel, or at least the wheel must cease to bite upon the rail; and to this cause, as well as to the reciprocating action of the two pistons, the author considers may be due some portion of that fish-tail motion which is familiar to railway travellers. The calculations show the danger to be increased as the diameter of the driving wheel is diminished, and they are unfavourable to the use of light engines.

March 20, 1851.

SIR BENJAMIN C. BRODIE, Bart., Vice-President, in the Chair.

A paper was in part read, entitled "On the Anatomy and Physiology of Salpa and Pyrosoma." By Thomas H. Huxley, Esq. Communicated by Ed. Forbes, Esq., F.R.S. Received February 26, 1851.

March 27, 1851.

SIR RODERICK I. MURCHISON, V.P., in the Chair.

The reading of Mr. Huxley's paper "On the Anatomy and Physiology of *Salpa* and *Pyrosoma*," commenced at the previous meeting, was concluded.

The object of the author in the present paper is to inquire into the true nature of the singular phænomena of reproduction in the

Salpæ, whose existence was first demonstrated by Chamisso twenty years ago, and which have formed the basis of the theory of "alternate generations."

The author refers to M. Krohn as the only writer who has previously entered thoroughly into this subject; but while he bears testimony to the extreme accuracy of M. Krohn's statements, he submits that, as the latter are published in a very condensed form only, and without figures, they cannot affect any value that may attach to his own independent researches.

The forms of *Salpa* examined were the *S. democratica* and *S. mucronata*.

The author first describes their outward form, and shows that they are so different in appearance and in some points of organization, as to fully warrant the assumption (if they belonged to any other family) that they are different species. He then proceeds to describe the various organs in detail; first, however, discussing the proper nomenclature of the sides and ends of these animals, a subject on which much confusion has prevailed. Particular attention is called to the existence of an organ hitherto undescribed—a cylindrical, elongated body, like an internal shell,—here termed the "*endostyle*," which lies in the dorsal sinus, and has hitherto been confounded with the "dorsal folds" of Savigny. A peculiar system of delicate transparent vessels, taking its origin in the stomach and ramified over the intestine, is described and its nature inquired into.

The organs of reproduction are next inquired into. The young in the *Salpa democratica* constitute a "Salpa-chain," and are shown to arise by *gemmation* from a tubular diverticulum of the vascular system of the parent. In the course of their development they take the form of the *S. mucronata*.

The young in the *Salpa mucronata* again is shown to be solitary, and attached to one point of the respiratory chamber of the parent by an organ which exactly represents in its structure a rudimentary mammiferous placenta, except that in the *Salpæ* the "villus" is formed by the maternal system, the "placental cell" by the fœtal system. But the fœtus here is not produced by *gemmation*, as in the preceding case, but by a true process of *sexual generation*.

Every *Salpa mucronata* contains at one period of its existence a solitary ovum, and a testis, which is a ramified gland surrounding the intestine, and hitherto confounded with the liver. The solitary ovum becomes fertilized, pushed out into the respiratory cavity of the parent, and remains connected with the latter until it has assumed the form of the *Salpa democratica*, when it becomes detached.

Chamisso's formula therefore, "that the parent *Salpa* produces an offspring different from itself, which again produces an offspring different from *itself*, but similar to its parent," is perfectly correct, only the word "produce" has two meanings—in the one case signifying *a process of gemmation*, in the other of *true sexual generation*.

The author next proceeds to describe the anatomy of *Pyrosoma*, and to point out its general harmony with that of *Salpa*. He shows

the existence of an endostyle—a system of ramified intestinal tubules—and of other organs precisely resembling those described in the latter genus. The "hepatic organ" of Savigny is the testis, while the female generative organ consists of solitary pedicillate ova. The arrangement of their parts is essentially the same as in *Salpa*, only that the fœtus does not appear to be developed in placental connexion with the parent.

The *Pyrosomata* increase by gemmation also, but the gemmæ are solitary and do not form chains, becoming developed like those of the ordinary compound Ascidians between the pre-existing forms.

In the next section, the zoological relations of the *Salpæ* and *Pyrosomata*, with the other Ascidians, are inquired into. The author endeavours to show that there is no essential difference of organization between the ordinary Ascidians and the *Salpæ*; that the two forms grade insensibly one into the other; and that there is, therefore, no ground for breaking up the great ascidian family into the two subdivisions of Monochitonida and Dichitonida.

With regard to the theory of the "alternation of generations," the author submits that it is by no means a proper expression for the phænomena presented by the *Salpæ*. According to the author's view, the two forms of *Salpa* are not two generations of distinct individuals, but are, properly speaking, organs, and only when taken together, equivalent to an individual, in the sense in which that term is used among the higher animals.

For these pseudo-individuals, in this and all analogous cases, the author proposes the name of "*zoöids*," simply for the purpose of avoiding the apparent paradox of calling these highly-organized independent forms "*organs*," though such, in the author's opinion, they really are.

The following letter, addressed to S. Hunter Christie, Esq., Sec. R.S., by James Glaisher, Esq., F.R.S., "On the Extraordinary Fall of Rain in the neighbourhood of London on the 15th instant," was read.

"13 Dartmouth Terrace, Blackheath.
1851, March 27.

"My dear Sir,—The fall of rain in the neighbourhood of London on the 15th instant was so remarkable, that I think an account of it will be interesting to the Fellows of the Royal Society.

"At Greenwich it commenced falling about 1 o'clock A.M., and by 9^h A.M. the amount fallen was 1 inch, and by 4 o'clock P.M. at the Royal Observatory 1·45 inch was measured; at Lewisham the fall was 1·725 inch; in London an inch nearly had fallen by 9^h A.M., and by 4^h P.M. the amount collected was 1·25 inch.

"These quantities are unusual at any season, but particularly so in the month of March; there is no record either in the MSS. of the Royal Observatory, or in the Philosophical Transactions, of so large a fall in any day in the month of March, and, so far as I can find, it is unprecedentedly large.

"The annexed table shows the amount of rain fallen on this day at various places in England and Ireland.

Names of Places.	Fall of rain in inches.	Wind. Direction.	Remarks.
Jersey	0·350	W.N.W.	
Guernsey	0·888	W.	
Helston	0·000	....	0^{in}·56 fell on Sunday the 16th.
Falmouth	0·047	W.N.W.	
Truro	0·150	W.N.W.	Showery: blowing fresh.
Exeter	0·180	N.W.	
Gosport	0·890	N.N.E.	Ceased raining at 7^h A.M.
Southampton	1·060	N.	The rain fell before 8^h A.M.
Midhurst	0·970	N.	
Uckfield	1·200	N.E.	The greater part fell in 6 hours.
Valentine Terrace	1·360	E.N.E.	The direction of the wind on March 14 was S.W. till 5^h P.M.; it then veered to S. by W. till 11 P.M.; it was S.E. at midnight; on the 15th at 1^h it suddenly changed to N.E., and during the time of the heavy fall of rain it was E.N.E.; at noon it was N., after which it was N.N.W.
Greenwich R. Obs.	1·450	E.N.E.	
Hyde Vale, Greenwich	1·55	E.N.E.	
Lewisham	1·725	E.N.E.	
Fleet Street, London	1·280	E.N.E.	
Chiswell Street, London	1·200	E.N.E.	
Westminster	1·00	E.N.E.	
St. John's Wood	1·044	E.N.E.	The rain ceased at 3^h P.M.
Hungerford	0·050	S. to S.W.	
Burnslade	0·180	S. to W.	
Foxhangers	0·100	W.	
Bradford	0·020		
Crofton	0·000	S. to W.	
Radcliffe Obs., Oxford	0·228	N.	Cloudy.
Rose Hill near Oxford	0·276	N.	Fine day, interrupted with flying clouds.
Linslade	0·520	N.	Rain ceased falling at noon.
Stone	0·490	N.	Rain falling all day; the Chiltern hills are covered by snow.
Hartwell	0·530	N.	
Cardington	0·345	N.E.	Rain all day.
Norwich	0·130	N.E.	Rain all day.
Holkham	0·160	E.	Thin rain occasionally all day.
Nottingham	0 00	N.E.	Misty: a fine day.
Grantham	0·00	calm.	Calm: foggy: dull.
Hawarden	0·00	S.	
Liverpool	0·006	S.	
Manchester	0·000	S.E.	
Wakefield	0·770	var.	
York	0·000	S. by W.	A brilliant day, almost continued sunshine.
North Shields	0·000	S.W.	A fine sunny day.
Durham	0·000	S.W.	Our falls of rain have been after those in the south.
Stonyhurst	0·074	E.S.E.	Fine and sunny during the day.
Whitehaven	0·252	W.S.W.	
Glasgow	0·080	E.N.E.	Rain.
Dunino near St. Andrews	0·000	S.S.W.	
Ireland.			
Cork	0·000	calm.	Sun was shining all day.
Bridgetown near Wexford	0·000	W.	Cloudy.
Ennis	0·100	N.W.	Showers: partially cloudy.
Longford	0·060	W. & N.W.	Showery.
Strokestown	0·045	N.	Forenoon showery, aftern. fine.
Carrick-on-Shannon	0·019	W.	Forenoon fair, aftern. showery.

"By reference to this table, it will be seen that the heavy fall extended over the counties of Middlesex, Kent, Sussex and Hamp-

shire, and that the direction of the wind over these counties during the time was chiefly N. and E.N.E. At many places the day was fine and bright.

"The change of wind from S. nearly to S.E. by midnight on the 14th, and to N.E. at about the time the fall began, and the change of wind just before the rain ceased to N.N.W., indicate that this great fall of rain in so short a time was attributable to the meeting of two currents of air of different temperatures, and the consequent great deposition of moisture.

"It will be seen that the weather in Ireland on this day was for the most part fine.

"I am, dear Sir,
"Yours very faithfully,
"James Glaisher."

"P.S. The average fall of rain in the month of March is about 1·7 inch."

"*S. H. Christie, Esq.,*
Sec. R. S."

April 3, 1851.

LIEUT.-COLONEL SABINE, R.A., V.P. & Treas. in the Chair.

A paper was read, entitled "Observations upon *Appendicularia* and *Doliolum*." By Thomas H. Huxley, Esq. Communicated by E. Forbes, Esq., F.R.S. Received Feb. 26, 1851.

This is a description of two Ascidian genera which possess very considerable interest: *Doliolum*, as forming a link between the *Pyrosomata* and *Salpæ*; and *Appendicularia*, as representing in a permanent form the larval state of the Ascidians, long ago described by M. Milne-Edwards.

Appendicularia, which has been also imperfectly described under the names of *Oikopleura* and *Venillaria*, is in fact an ascidian provided with a long vibratile fin or tail, by the aid of which it swims freely about.

That it is an adult form is shown by the existence of a well-developed testis, but the author leaves undecided the nature of the female organs.

On the other hand it seems doubtful whether *Doliolum* is truly an independent form, or whether it is not rather a detached "zoöid" of the genus *Auchenia*.

A paper was also in part read, entitled "Researches into the Molecular Constitution of the Organic Bases." Part II. By A. W. Hofmann, Ph.D. Communicated by Professor Graham, F.R.S. Received March 12, 1851.

April 10, 1851.

SIR PHILIP DE MALPAS GREY EGERTON, Bart., V.P., in the Chair.

The reading of Dr. Hofmann's paper " On the Molecular Constitution of the Organic Bases," was resumed and concluded.

In a former paper* the author advanced a general theory regarding the constitution of the *volatile* organic bases. He showed that in all these substances the original structure of ammonia may be traced without difficulty, and that they must be viewed as ammonia in which either one, two, or the three equivalents of hydrogen are replaced by a corresponding number of compound molecules; he accordingly distinguished the volatile bases as amidogen, imidogen, and nitrile bases.

$$\left.\begin{array}{l}H\\H\\H\end{array}\right\}N \qquad \left.\begin{array}{l}V\\H\\H\end{array}\right\}N \qquad \left.\begin{array}{l}V\\X\\H\end{array}\right\}N \qquad \left.\begin{array}{l}V\\X\\Y\end{array}\right\}N$$

Ammonia. Amidogen bases. Imidogen bases. Nitrile bases.

In his present memoir he goes a step further, and communicates the discovery of a new group of alkaloids, closely connected by their origin and composition with the former class, but differing from it altogether by their properties. These new alkaloids are no longer *volatile* without decomposition, and form in this manner the connecting link between the artificial bases, nearly all of which are volatile, and the natural alkaloids, the majority of which are of a fixed nature.

The alkaloids of the new class, to which for the sake of convenience the author assigns the term of ammonium bases, arise from the nitrile bases by the assimilation of the additional equivalent of one of the replacing molecules in conjunction with oxygen and water.

$$\left.\begin{array}{l}V\\X\\Y\end{array}\right\}N+ZO,\ HO=\left.\begin{array}{l}V\\X\\Y\\Z\end{array}\right\}NO,\ HO.$$

In his memoir the author establishes the conditions under which this transformation takes place, viz. the action of the alcohol iodides on nitrile bases, and he developes the generality of his observations by the description of about a dozen of new alkaloids prepared according to his method; he points out moreover that the number which may be actually produced is nearly unlimited.

The new substances which are formed under the above conditions, are endowed with very remarkable properties. The alkaloid which is formed by the union of four ethyl equivalents with nitrogen, oxygen and water, the substance in which $V=X=Y=Z=C_4H_5$, *i. e.*

$$\left.\begin{array}{l}C_4H_5\\C_4H_5\\C_4H_5\\C_4H_5\end{array}\right\}NO,\ HO,$$

is thus described by the author.

* Phil. Trans., 1850, part i. p. 93.

It is of a powerfully alkaline reaction, which manifests itself, not only in its deportment with vegetable colours, but also in its combining their pungent taste with the bitterness of quinine. The solution, when concentrated, not only burns the tongue, but it acts strongly upon the epidermis, which it destroys, like caustic potassa or soda; on rubbing a solution of the oxide between the fingers, we feel the well-known sensation produced by the fixed alkalies under the same circumstances, and we perceive moreover the same peculiar odour. Oxide of tetræthylammonium, as the author calls this substance, saponifies the fats as readily as potassa. The experiment was made with cocoa-nut oil, which, after half an hour's ebullition with the new alkali, was converted into a beautiful soft soap, having the appearance of an ordinary potassa soap.

The author has traced the analogy of the new compound with potassa in many other directions, and especially in its deportment with the metallic oxides: his alkaloid, in fact, produces with the salts of the metals exactly the same reactions as potassa.

The other substances belonging to this group have similar properties, modified only by the character of the replacing molecules. Among them are several which are remarkable for the complex nature of their constitution, which, nevertheless, is perfectly intelligible.

In conclusion, the author draws attention to the assistance which the solution of the grand problem of preparing artificially the natural alkaloids, especially those of the cinchona bark, may receive from a further extension of these researches. He shows that even now the analogy in the general deportment of his ammonium bases, and the alkaloids of nature, is very great, although they differ materially in many properties.

A paper was read, entitled "Description and purpose of the glass plate which bears the inscription 'Interferenz-spectrum. Longitudo et celeritas undularum lucis relativa cum in aëre tum in vitro.'" By T. A. Nobert. Communicated by Michael Faraday, Esq., F.R.S. &c. Received April 10, 1851.

A rectangular plate of polished glass, 18 lines (Paris) in length, 9 lines in breadth, and 1·5 line in depth, has one of its longer narrow faces ground so as to form an angle of 72° with its upper surface. This narrow face is polished, the other three being ground at right angles to the upper and under surfaces. On the upper surface, at about the distance of 2 lines from the acute dihedral angle, twelve systems of extremely fine parallel lines are cut, so that in each system the lines are at perfectly equal distances, and the systems lie separate from one another. Over these lines lies a thin covering plate of glass, but at the place where the ruling is, a stratum of air is contained between the plates. In these twelve systems, named A, B, C, D, E, F, G, H, I, K, L and M, the respective distances of the lines measured with great accuracy in Paris lines, are as follow:—

for A = 0·0003375 line.
B = 0·0003063
C = 0·0002625
D = 0·0002438
E = 0·0002250
F = 0·0002063
G = 0·0001873
H = 0·0001750
I = 0·0001625
K = 0·0001500
L = 0·0001375
M = 0·0001281

In order to use the plate, it is to be placed on the table of a compound microscope having a magnifying power of 40 to 50 times, the table of the microscope being previously covered with black cloth to prevent the intrusion of all foreign light. The small covering plate being placed upwards, the polished face forming an acute angle with the upper side, is to be turned towards the window supplying light. The illumination must take place in a plane at right angles to the length of the plate, and at an angle of 18° inclination to the plane of the plate. The light of strongly illumined clouds, or of a point of the heavens near to the sun, is to be employed: direct sunlight is not recommended. A greater angle of incidence is of no consequence when making a preliminary observation; but it is necessary, in order to obtain strongly luminous colours, to turn the plate a little in azimuth while examining the lines, thereby producing the brightest illumination. If these preliminary precautions be taken, there will appear in the ruled space,—

for A, deep red;
B, orange;
C, yellow;
D, green;
E, light blue;
F, indigo;
G, violet.

If the foregoing numerical values for the systems A, B, C, D, E, F and G, be multiplied by cos 18°, the length of the undulations of light *in air* for those colours which are seen in the rulings will be obtained. This method is considered to admit of great exactness with this small angle of incidence, because its cosine varies but little.

As in the following five systems, H, I, K, L and M, no colour is perceived, the distances between the lines being there smaller than the lengths of the violet light-waves in air, the prediction of the undulatory theory is confirmed.

The plate is next to be turned so that the small covering plate, and, therefore, also the side of the glass upon which the lines are drawn, is downwards, and the polished narrow face towards the light,

which, as before, is incident at an angle of 18° on the plate. In this position, the light falling perpendicularly on the narrow face, passes through it unbroken, and being within the glass reflected from the rulings, the coloured pictures of all the twelve systems are now exhibited in the microscope. If the colours which these systems now show, and which have been formed *in the glass*, be compared with the former colours *in the air*, the system F, which is deep red, harmonizes with the system A in the air spectrum, and the systems G, H, I, K, L and M in the glass spectrum, according to their order and colour, correspond to the systems B, C, D, E, F and G in the air spectrum; and the author considers that the comparison of the foregoing values in the systems—

A and F,	E and K,
B and G,	F and L,
C and H,	G and M
D and I,	

shows that the lengths of undulation for the same colour in the air and in the glass are in the ratio 1·53 to 1, which is exactly the index of refraction of this glass. He further remarks, that, as the same period of undulation belongs to the same colour, the velocities of propagation in air and in the glass must be in the ratio of the mean value of the distances of the lines in the first seven systems, A, B, C, D, E, F, G, to the mean value of the distances of the lines in the last seven systems, F, G, H, I, K, L, M, or as 1·53 to 1; and that both results agree perfectly with the deductions from the wave-theory of light. In conclusion, the author points out the extraordinary degree of accuracy required in drawing these lines. It is stated that if inequalities amounting only to ·000002 line occur in the systems F, G, H, I, K, L, M, stripes of another colour will appear in them; and if the distance of the lines in M be diminished by that quantity, all colour disappears.

The following communication was also read:—

Extract of a letter from Professor Kämtz to Lieut. Colonel Sabine, on "Corrections of the Constants in the general theory of Terrestrial Magnetism." Received April 3, 1851.

Translation.

Dorpat, $\frac{4}{16}$ January 1851.

From the active zeal with which you pursue the phenomena of terrestrial magnetism, and collect all the facts which can conduce to the elucidation of this difficult subject, I think that some researches with which I have occupied myself will not be wholly uninteresting to you; and I therefore address you the following lines, which I have also permitted myself to write in my own language.

Some years ago I employed myself in endeavouring to correct the constants which Gauss has given for the earth's magnetism. The process I adopted was by considering the horizontal and vertical components separately; but when I learned that Erman had the

same work in hand, I left mine unfinished. I did not then possess the Reports of the British Association, as it was not until this last summer (1850) that they were obtained here, and when I had seen Erman's results, I at once decided on taking up my work afresh. I have made use of all the data I could procure, and have thus been able to determine the component Z at above 1400 places, including a series of observations which I had myself made from 1847 to 1849 in Liefland, Esthonia, Finland, Norway, and on the route from Archangel to Petersburg. I have as far as possible reduced all determinations to the epoch of 1830. A calculation of the several observations by the method of least squares would have required an entire life; I therefore preferred following the same path as Gauss; in doing this, however, I soon discovered that the 5th order could not be neglected; and I then obtained the following values:—

$g^{1,0}= 927{\cdot}9$	$g^{1,1}= 89{\cdot}8$	$h^{1,1}=-163{\cdot}7$	$g^{2,2}= 2{\cdot}5$	$h^{2,2}=-37{\cdot}3$
$g^{2,0}=-6{\cdot}4$	$g^{2,1}=-140{\cdot}6$	$h^{2,1}=-14{\cdot}1$	$g^{3,2}=-86{\cdot}9$	$h^{3,2}=-17{\cdot}2$
$g^{3,0}=-51{\cdot}8$	$g^{3,1}= 112{\cdot}3$	$h^{3,1}= 48{\cdot}5$	$g^{4,2}=-41{\cdot}3$	$h^{4,2}= 43{\cdot}4$
$g^{4,0}=-83{\cdot}2$	$g^{4,1}=-103{\cdot}2$	$h^{4,1}=-18{\cdot}2$	$g^{5,2}=-96{\cdot}5$	$h^{5,2}=-10{\cdot}0$
$g^{5,0}= 14{\cdot}3$	$g^{5,1}=-115{\cdot}1$	$h^{5,1}= 72{\cdot}8$		

$g^{3,3}=-4{\cdot}4$	$h^{3,3}=-25{\cdot}1$	$g^{4,4}=3{\cdot}9$	$h^{4,4}=4{\cdot}3$
$g^{4,3}= 18{\cdot}8$	$h^{4,3}= 18{\cdot}6$	$g^{5,4}= {\cdot}0$	$h^{5,4}=2{\cdot}8$
$g^{5,3}= 3{\cdot}3$	$h^{5,3}=-1{\cdot}6$		
		$g^{5,5}= {\cdot}0$	$h^{5,5}= {\cdot}0$

A comparison will show you that these quantities agree much better with Gauss's than Erman's do; and this is also true in respect to the agreement with the observations, especially in the high south latitudes. Thus there was found—

	Latitude.	Longitude.	Inclination.	Force.
	° ′	° ′	° ′	
	−69 54	179 55	−84 30	1999
	−69 52	180 04	−83 34	1994
Means	−69 53	180 0	−84 02	1996·5

Z=−1985·8 for −70° and 180°; Gauss found −2193·5; Erman −1781·1; my calculation gives −2009·3. My constants also still require a small correction. I do not however mean to examine this at present, but propose first to consider the horizontal component, in order to satisfy myself previously whether both components depend upon the same constants or not. The probable error of a single determination is nearly 19; and to show the degree of agreement, I subjoin the following table. As in forming it I merely took from my large table every 10th observation in the order of succession, you will not be surprised at finding unimportant places, whilst others of greater note in their vicinity are omitted: it may suffice however for the present purpose. The quantities given are the differences between the observed and calculated vertical intensity.

Stations.	Lat. N.	Long. E.	Δ Z.	Observers.
	° ′	° ′		
1. Fairhaven, Spitzbergen	79 40	11 40	− 24·1	Sabine.
11. Tromsöe	69 39	18 55	+ 31·2	Keilhau.
21. Tukansk. Isl.	68 4	39 35	− 39·1	Reinicke.
31. Grundsät	60 56	11 35	+ 39·8	Hansteen.
41. Sundsvall	62 22	17 16	+ 2·5	Hansteen.
51. Abo	60 27	22 18	− 15·7	Hansteen.
61. Danzig	54 21	18 38	+ 31·0	Ericksen.
71. Doskino	56 9	43 34	− 32·8	Erman and Hansteen.
81. Perm	58 1	56 14	− 30·0	Erman and Hansteen.
91. Tiumen	57 10	65 27	− 13·4	Erman and Hansteen.
101. Wandiask	66 16	65 10	− 27·7	Erman.
111. Tschuluim	55 6	81 14	− 24·6	Erman.
121. Botowsk	55 10	105 22	− 3·7	Erman.
131. Monachanowa	50 58	106 29	+ 6·8	Hansteen.
141. Nowaja River	72 7	95 25	+ 22·3	Middendorf.
151. Progromnoi	52 30	111 3	+ 11·8	Fuss.
161. Nalaicha	47 47	107 18	+ 9·8	Fuss.
171. Chapchaktu	46 2	108 35	− 13·0	Fuss.
181. Zackildack	42 48	114 17	+ 13·5	Fuss.
191. Gaschun	44 23	111 19	− 9·8	Fuss.
201. Arki	60 6	142 20	+ 1·2	Erman.
211. Sitka	57 3	224 34	− 2·2	Lütke, Erman, Belcher.
221. F. Dunvegan	55 56	241 26	+ 22·7	Lefroy.
231. Frog Portage	55 28	256 30	+ 9·1	Lefroy.
241. York Factory	57 0	267 34	+ 4·9	Lefroy.
251. Fort Alexander	50 37	263 39	+ 13·4	Lefroy.
261. Devil's Drum Island	53 19	259 20	− 22·7	Lefroy.
271. Cape Disappointment	46 16	236 4	− 34·2	Douglas.
281. Lac à la Pluie	48 32	267 4	+ 19·9	Lefroy.
291. Fort à la Cloche	46 7	277 35	− 19·7	Lefroy.
301. Portage Ecarté	48 25	270 15	+ 4·8	Lefroy.
311. Chat Falls	45 26	283 28	+ 22·8	Lefroy.
321. Pointe aux Chênes	45 37	285 5	+ 7·7	Lefroy.
331. Lake Nipissing	46 13	280 1	+ 16·7	Lefroy.
341. Waterville	44 33	293 23	+ 11·9	Keely.
351. Dubuque's Town	42 29	269 37	+ 20·8	Locke.
361. St. Mary's	40 32	275 41	+ 31·9	Locke.
371. Detroit	42 25	277 0	+ 24·0	Loomis, Younghusband, Locke, [Lefroy.
381. Alleghany Summit	40 27	281 50	+ 29·3	Locke.
391. Utica	43 7	284 47	+ 27·2	Loomis, Locke.
401. Portland	43 41	289 40	+ 14·5	Locke.
411. St. Louis	38 38	269 56	+ 35·8	Locke, Loomis, Nicollet.
421. Paoli	38 5	273 35	+ 30·7	Locke.
431. Columbus	39 57	276 57	+ 11·9	Locke.
441. Lerwick	60 9	358 53	+ 46·8	Ross.
451. Loch Slapin	57 14	353 58	+ 30·4	Sabine.
461. Braemar	57 1	356 35	+ 16·2	Sabine.
Edinburgh	....	....	+ 8·8	
471. Valencia	51 56	349 43	+ 8·5	Sabine, Ross.
481. Enniskillen	54 21	352 22	+ 25·6	Lloyd.
491. York	53 58	358 54	+ 22·5	Phillips, Ross.
501. Calderstone	53 23	357 7	+ 17·7	Phillips.
511. Castleton	54 4	355 20	+ 22·7	Phillips.
Dublin	....	....	+ 15·2	All Observers.
521. Fermoy	52 7	351 44	+ 13·5	Sabine.
531. Clifton	51 27	357 25	+ 16·4	Lloyd, Ross.

Stations.	Lat. N.	Long. E.	Δ Z.	Observers.
	° ′	° ′		
541. London	51 31	359 53	+ 21·9	All Observers.
551. Salisbury	51 4	358 12	+ 18·3	Lloyd, Ross.
561. Dover	51 8	1 19	+ 19·7	Sabine.
571. Fontainebleau	48 24	2 38	+ 11·0	Fox.
581. Nimes	43 50	4 20	− 3·8	Fox, Humboldt.
591. Malaga	36 44	355 36	− 13·4	Norwegian Officers.
601. Prague	50 5	14 27	+ 15·5	Keilhau, Kreil.
611. Berne	46 57	7 25	+ 12·0	Fox.
621. Seelau	49 32	15 17	+ 21·6	Kreil.
631. Rome	41 54	12 26	+ 5·0	Humboldt, d'Abadie, Quetelet.
641. Milo	36 43	24 27	+ 19·6	Norwegian Officers.
651. San Diego	32 41	242 27	− 70·6	Belcher.
661. At sea	47 7	346 54	− 0·4	Sulivan.
671. At sea	44 22	330 54	− 18·3	Erman.
681. At sea	30 0	318 5	− 4·3	Sulivan.
Teneriffe	28 27	343 43	− 0·4	Humboldt, Freycinet, Duperrey, Sabine, Bethune, Wickham, Sulivan.
691. At sea	21 32	316 43	+ 21·3	Sulivan.
701. At sea	23 12	238 9	− 61·2	Erman.
711. Socorro Island	18 43	249 6	+ 28·0	Belcher.
721. Ulean	7 22	143 57	− 10·8	Lütke.
731. At sea	8 55	235 48	− 27·8	Erman.
741. La Guayra	10 36	292 54	+ 2·6	Humboldt.
751. Morales	8 15	286 0	− 7·0	Humboldt.
761. At sea	10 7	319 51	+ 48·9	Sulivan.
771. St. Thomas, Fernando Po, Isla das Rolhas	1 23	7 20	− 35·8	The secular change at this station is uncertain; I take the mean of the inclinations by Sabine and Allen; the force at St. Thomas, from Sabine.
781. At sea	3 47	162 59	+ 8·2	Lütke.
791. At sea	− 2 2	236 4	+ 31·7	Lütke.
801. Pasto	1 13	282 39	− 13·0	Humboldt and Bousingault.
811. At sea	5 45	331 9	+ 8·4	Erman.
821. At sea	5 37	341 3	+ 10·8	Dunlop.
831. Pulo Kumpal	− 2 44	110 7	− 16·8	Belcher.
841. Shell Rock	− 1 57	136 21	+ 1·4	Belcher.
851. Gonzanama	− 4 13	280 27	− 4·5	Humboldt.
861. At sea	− 1 10	223 32	− 19·9	Erman.
871. Tomependa	− 5 31	281 24	+ 10·1	Humboldt.
881. Huaura	−11 3	282 14	+ 12·9	Humboldt.
891. At sea	− 0 27	324 44	+ 52·2	Sulivan.
901. At sea	− 8 10	339 50	− 21·1	Dunlop.
911. At sea	−11 54	214 37	− 9·4	Erman.
921. At sea	−13 9	251 20	+ 57·0	Lütke.
931. Bow Island	−18 5	219 7	+ 7·5	Belcher.
941. At sea	−19 56	325 5	− 20·7	Sulivan.
St. Helena	−15 55	354 17	+ 14·7	All Observers.
951. At sea	−26 25	49 12	− 17·1	Moore and Clerk.
961. At sea	−21 54	53 0	− 20·0	Moore and Clerk.
Mauritius	−20 9	57 31	− 0·5	Duperrey, Fitzroy, Moore and Clerk.
971. At sea	−22 41	69 54	− 26·7	Dayman.
981. At sea	−22 38	76 10	− 17·4	Dayman.
991. At sea	−22 34	80 10	− 22·1	Dayman.

Stations.	Lat. S.	Long. E.	Δ Z.	Observers.
	° ′	° ′		
1001. At sea	24 17	94 6	− 7·5	Moore and Clerk.
1011. At sea	21 51	268 5	+ 19·0	Lütke.
1021. At sea	29 53	313 43	− 19·3	Erman.
1031. At sea	38 44	0 16	− 7·7	Dunlop.
1041. At sea	35 48	18 47	− 25·8	Erebus and Terror.
1051. At sea	32 17	29 34	+ 0·6	Dayman.
1061. At sea	38 11	22 0	− 29·9	Erebus.
1071. At sea	39 16	30 27	− 10·7	Dunlop.
1081. At sea	33 47	111 4	+ 31·3	Dayman.
1091. At sea	35 5	117 56	− 7·1	Moore and Clerk.
1101. At sea	42 35	125 40	+ 51·3	Smith.
Sydney	....	....	+ 39·0	All Observers.
			+ 17·7	(British only).
1111. At sea	33 38	163 42	− 33·2	Erebus.
1121. Bay of Islands	35 16	174 0	− 8·4	Duperrey, FitzRoy, Erebus.
1131. Valdivia	39 53	286 31	+ 41·3	FitzRoy.
1141. At sea	44 4	312 1	+ 9·1	Sulivan.
1151. At sea	37 37	353 36	− 36·3	Dunlop.
1161. At sea	41 47	26 38	− 16·7	Erebus.
1171. At sea	46 28	52 31	− 4·0	Erebus. Maximum of probable error. There are great anomalies in this meridian.
1181. At sea	48 40	68 58	− 55·5	Erebus.
Kerguelen Island	48 41	68 54	− 11·9	Erebus.
1191. At sea	47 39	103 42	− 23·4	Erebus.
1201. At sea	47 34	124 43	− 109·6	Erebus.
Hobart Town	42 53	147 24	+ 41·8	All Observers.
1211. Bass's Strait	40 28	151 35	+ 11·5	Wickham.
1221. At sea	41 49	183 41	− 18·3	Erebus.
1231. At sea	49 23	188 29	·0	Erebus.
1241. At sea	53 57	6 5	− 4·1	Moore and Clerk.
1251. At sea	54 55	132 50	− 53·8	Erebus.
1261. At sea	57 54	170 25	+ 40·4	Erebus.
1271. At sea	53 1	205 8	− 18·3	Erebus.
1281. At sea	58 39	213 17	− 6·7	Erebus.
1291. At sea	60 21	237 54	− 19·0	Erebus.
1301. At sea	58 25	279 44	+ 10·8	Erebus.
Port Famine	53 38	289 2	+ 4·6	King and FitzRoy.
1311. At sea	46 0	299 50	− 49·2	Sulivan.
Falkland Islands	51 33	301 55	+ 41·3	All Observers.
1321. At sea	61 10	9 5	+ 15·9	Moore and Clerk.
1331. At sea	66 33	36 48	+ 17·5	Moore and Clerk.
1341. At sea	66 24	40 30	+ 8·1	Moore and Clerk.
1351. At sea	60 50	87 41	+ 10·1	Moore and Clerk.
1361. At sea	65 9	143 7	− 52·5	Erebus.
1371. At sea	64 41	162 34	− 10·5	Erebus.
1381. At sea	61 34	170 40	+ 26·7	Erebus.
1391. At sea	67 14	188 6	+ 18·4	Erebus.
1401. At sea	65 18	191 39	+ 28·5	Erebus.
1410. At sea	67 16	202 13	+ 30·4	Erebus.
1411. At sea	61 15	213 54	+ 22·2	Erebus.
1412. At sea	62 38	212 44	+ 3·4	Erebus.
1421. At sea	70 23	174 50	− 10·2	Erebus.
1431. At sea	72 58	189 50	+ 21·0	Erebus.
1441. At sea	77 6	192 31	− 8·0	Erebus.
1444. At sea	77 47	197 25	+ 23·2	Erebus.

I think the agreement pretty good for a calculation which I still expect to correct in some degree; it is also to be remarked that I

have taken the results of all observers, and that their determinations often differ considerably from each other at the same place. Unfortunately I could not make use of the two important determinations of the Euphrates Expedition for want of the Inclination.

As you collect everything that can serve towards a final determination of the elements, I permit myself to subjoin the following data which are still partly unpublished.

Stations.	Lat. N.	Long. E.	Date.	Inclination.		Horizontal Force.	Total Force.	Vertical Force.		
				Observed.	Reduced to 1830.			Observed.	Calculated.	Difference.
Uellenorm	58° 19′	26° 43′	1847.	70° 9′·8	70° 38′·0	473·7	1396·0	1317·0	1317·6	− 0·6
Dorpat*	58 23	26 44	1847 to 1850.	70 50·7	71 19·9	465·4	1421·9	1347·1	1318·0	+29·1
Kardis	58 51	26 17	1847.	70 17·1	....	471·6				
			1849.	17·5	....	467·1				
				70 17·3	70 48·3	469·3	1388·4	1311·2	1323·2	−12·0
Revel	59 35	24 43	1847.	70 54·4						
			1849.	45·8						
				70 50·1	71 21·1	454·4	1384·2	1311·5	1330·7	−19·2
Nawast	58 35	25 34	1848.	70 41·0	71 12·0	454·7	1374·5	1301·2	1318·6	−17·4
Werder†	58 35	23 40	1848.	69 31·6	70 2·6	484·6	1385·4	1302·2	1315·9	−13·7
Arensburg	58 15	22 25	1848.	70 51·1	71 22·1	455·5	1388·8	1316·0	1309·1	+ 6·9
Kabbil	58 20	22 40	1848.	71 9·3	71 40·3	437·6	1354·8	1286·1	1310·4	−24·3
Pernaw	58 22	24 32	1848.	70 36·3	71 7·3	458·4	1380·1	1305·9	1313·9	− 8·0
Tammiss	58 21	24 33	1848.	70 24·5	70 55·5	459·0	1368·8	1293·6	1313·7	−20·1
Kurkundt	58 8	24 59	1848.	69 47·9	70 18·9	476·2	1378·7	1298·0	1311·8	−13·8
Helsingfors	60 10	24 57	1847.	71 21·7	....	444·3				
			1849.	19·7	....	7·6				
				71 20·7	71 51·7	446·0	1394·3	1325·0	1339·0	−14·0
Bollstad	60 9	24 13	1847.	71 30·2	71 59·4	441·4	1391·4	1323·2	1338·0	−14·8
Kyrkstad	60 10	24 5	1847.	71 21·9	71 51·1	442·6	1385·1	1316·2	1338·0	−21·8
Lambola	60 15	23 10	1847.	71 28·9	71 58·1	442·7	1393·8	1325·4	1335·8	−10·4
Nukari	60 22	24 55	1847.	71 40·3	72 9·5	440·8	1401·8	1334·3	1341·2	− 6·9
Abborfors	60 30	26 30	1847.	71 19·8	71 49·0	450·0	1406·2	1335·9	1346·0	−10·1
Grönwick.........	60 33	27 30	1847.	71 32·3	72 1·5	441·1	1393·1	1325·0	1348·5	−23·5
Wiborg	60 44	28 50	1847.	70 51·6	71 20·8	446·2	1360·9	1289·4	1353·0	−63·6
Turkhauta	60 50	24 47	1847.	72 14·6	72 43·8	425·5	1395·1	1332·2	1346·5	−14·3
Tavastehus	61 0	24 28	1847.	72 8·4	72 37·6	427·6	1394·7	1331·1	1348·0	−16·9
Wilmanstrand...	61 4	28 16	1847.	71 51·8	72 21·0	439·0	1410·2	1344·0	1356·1	−12·1
Imatra Fall	61 11	28 55	1847.	71 51·0	72 20·2	433·6	1411·3	1344·8	1357·6	−12·8
Huutjarwi	61 28	24 2	1847.	72 2·3	72 31·5	433·5	1405·8	1340·9	1352·9	−12·0
Pumala............	61 32	28 15	1847.	72 7·9	72 37·1	431·7	1406·9	1342·6	1361·7	−19·1
Wehuwarpe... ..	61 46	22 49	1847.	72 6·4	72 35·6	433·0	1409·3	1344·9	1353·8	− 8·9
Nyslott............	61 52	29 0	1847.	71 59·9	72 29·1	437·2	1414·6	1349·1	1367·3	−18·2
Tjök‡	62 18	21 23	1847.	72 43·0	73 12·2	419·4	1411·7	1351·3	1357·9	− 6·6
Warkauss-Sluss..	62 20	27 58	1847.	72 32·4	73 1·6	420·1	1400·1	1339·1	1371·0	−31·9
Johannisdal	62 21	21 21	1847.	73 27·3	73 56·5	399·4	1402·6	1347·9	1358·6	−10·7
Kuopio............	62 55	27 33	1847.	72 54·3	73 23·5	415·5	1413·6	1354·6	1377·2	−22·6
Wasa§	63 5	21 35	1847.	73 0·8	73 30·0	411·7	1442·0	1351·0	1367·6	−16·6

* In the garden near my house, and at different parts of the town and its environs; including differences of inclination of more than 1° 15′.

† H. F. very anomalous. ‡ Hansteen, 1825, $\Delta Z = -12{\cdot}4$.

§ Hansteen, 1825, $\Delta Z = -13{\cdot}3$.

Stations.	Lat. N.	Long. E.	Date.	Inclination. Observed.	Inclination. Reduced to 1830.	Horizontal Force.	Total Force.	Vertical Force. Observed.	Vertical Force. Calculated.	Vertical Force. Difference.
	° ′	° ′		° ′	° ′					
Sawojarwi	63 22	27 13	1847.	72 53·1	73 22·3	434·6	1511·1	1415·0	1383·0	+32·0
Sundby	63 36	22 40	1847.	73 18·9	73 48·1	401·8	1399·4	1343·9	1375·0	−31·1
Aho	64 2	26 27	1847.	73 24·9	73 54·1	407·4	1427·4	1371·5	1388·0	−16·5
Wirda	63 37	27 3	1847.	73 9·4	73 38·6	411·5	1420·1	1362·6	1384·6	−22·0
Salahmi	63 47	27 0	1847.	73 14·2	73 43·4	408·8	1417·2	1360·6	1386·6	−26·0
Kyrola	64 5	23 30	1847.	73 24·8	73 54·0	409·0	1432·7	1376·5	1382·6	− 6·1
Tuomala	64 25	26 0	1847.	73 30·7	73 59·9	410·7	1446·7	1390·7	1389·8	+ 0·9
Lassila	64 45	24 38	1847.	73 50·5	74 19·7	408·2	1466·9	1412·4	1393·0	+19·4
Uleaborg*	65 3	25 27	1847.	74 6·0	74 35·2	393·2	1435·2	1383·6	1398·6	−15·0
Wuornos	65 36	25 26	1847.	74 4·8	74 30·0	393·5	1434·7	1382·4	1405·3	−22·9
Rautiola	65 47	24 40	1847.	74 49·9	75 19·1	377·5	1437·9	1390·9	1405·2	−14·3
Tornea............	65 52	23 30	1847.	74 52·3		380·2				
			1849.	48·4		2·6				
				74 50·3	75 19·5	381·4	1458·4	1410·9	1404·1	+ 6·8
Haaparanda† ...	65 52	23 30	1849.	74 28·1	74 57·3	382·1	1427·0	1378·1	1404·1	−26·0
Alkula ‡	66 20	23 49	1847.	74 21·1		392·2				
			1849.	15·2		3·8				
				74 18·2	74 47·4	393·0	1452·8	1401·9	1410·0	− 8·1
Toluanen ‡	66 36	23 52	1847.	74 31·9	75 1·1	386·9	1450·3	1401·1	1413·3	−12·2
Turtola‡	66 42	23 40	1847.	74 47·7	75 16·9	381·3	1453·9	1406·1	1414·5	− 8·4
Kardis Lappl.‡...	67 0	23 39	1847.	75 4·4	75 33·6	374·3	1452·9	1407·1	1418·0	−10·9
Kexiswaara‡ ...	67 15	23 27	1847	75 45·2	76 14·4	366·1	1487·8	1445·1	1420·6	+24·5
Muonionisca‡...	68 0	23 42	1847.	75 32·0		364·7				
			1849.	31·0		5·6				
				75 31·5	75 59·7	365·2	1459·1	1415·7	1430·2	−14·5
Kätkesuando ‡...	68 7	23 22	1849.	75 32·1	76 1·3	359·8	1440·6	1397·9	1430·7	−32·8
Palajoensu	68 18	22 45	1849.	76 5·7	76 34·9	350·0	1456·7	1416·9	1432·5	−16·6
Kaaressuando ...	68 24	22 8	1849.	75 37·1	76 6·3	359·3	1446·6	1404·3	1433·7	−29·4
Kielli-jarwi	69 5	20 40	1849.	75 52·4	76 21·6	355·1	1455·0	1414·0	1439·0	−25·0
Tromsöe§.........	69 39	18 56	1849.	76 11·4	76 40·6	348·1	1458·4	1419·2	1444·4	−25·2
Hammerfest ‖ ...	70 40	23 45	1849.	76 43·8	77 13·0	344·3	1500·2	1463·0	1464·0	− 1·0
Havösund¶	71 0	24 45	1849.	76 46·1	77 15·3	336·7	1471·0	1434·8	1466·1	−31·3
Kielwig Mageroe	70 57	26 15	1849.	76 54·6	77 23·8	333·7	1473·5	1438·0	1467·5	−29·5
Kitai-Insel** ...	68 28	38 30	1849.	75 50·6	76 9·6	358·2	1464·6	1422·0	1476·7	−54·7
Archangel†† ...	64 30	40 33	1849.	73 58·8	74 8·3	405·4	1468·9	1413·0	1439·1	−26·1
Bobrowsk	64 28	41 0	1849.	74 1·5	74 11·0	404·5	1469·6	1414·0	1440·8	−26·8
Kaduish	62 55	41 30	1849.	73 19·6	73 29·1	420·2	1464·5	1404·1	1422·8	−18·7
Plesskaja	62 35	40 55	1849.	72 46·7	72 57·2	429·8	1451·7	1387·9	1408·3	−20·4
Krassnowskaja...	62 10	40 10	1849.	72 33·5	72 43·0	432·8	1443·8	1378·7	1407·3	−28·6
Ustwelskoi	61 55	39 12	1849.	72 15·3	72 25·3	442·1	1450·6	1382·8	1400·4	−17·6
Kargopol	61 43	38 57	1849.	72 8·2	72 19·2	444·4	1448·6	1380·2	1395·7	−15·5
Badoshkaja	60 48	37 30	1849.	71 25·3	71 28·6	459·0	1440·8	1366·1	1381·1	−15·0
Wytegra	61 1	36 28	1849.	71 34·2	71 48·2	457·1	1445·9	1373·6	1380·5	− 6·9
Gomorowitschi...	60 55	34 35	1849.	71 34·4	71 53·0	450·6	1425·5	1354·8	1371·0	−16·2
Petersburg ‡‡ ...	59 56	30 18	1849.	70 33·2	70 59·0	473·1	1420·8	1343·2	1347·0	− 3·8

* Hansteen 1825, $\Delta Z = -12{\cdot}0$. † Hansteen 1825, $\Delta Z = -12{\cdot}1$.
‡ Hansteen 1825, $\Delta Z + 0{\cdot}1$; many iron mines in the vicinity; quantities of magnetic ironsand on the banks of Tornea river. § Keilhau, $\Delta V + 31{\cdot}2$.
‖ Sabine, $+2{\cdot}8$; Keilhau, $-30{\cdot}9$. ¶ Keilhau, $-27{\cdot}4$. ** Keilhau, $-3{\cdot}2$.
†† Reinicke and Mailander, $-62{\cdot}5$.
‡‡ Inclination observed by me; force by Kupffer; earlier observations gave $\Delta Z = -9{\cdot}3$.

In the above table, the horizontal force was obtained by vibrations, and reduced to 0° Reaumur. Before and after my journey in 1847, the force was determined at Dorpat by Gauss's method, and the needle employed compared therewith and reduced to the intensity in London=1372. Subsequently I preferred for trying the needles, Poisson's method, at least for traveling purposes; but some alterations require to be introduced in Poisson's formula, as he has overlooked some things. With the same needle which I employed in both my journeys, I have made more than 60 determinations of absolute force at Dorpat, partly in a room and partly in the open air, and in temperatures varying from −13° R. to + 25° R., and have found a very good accordance. I also made several such determinations in the journeys of 1848 and 1849.

As I do not possess an observatory, and cannot employ a Bifilar in my dwelling-house, it has not been possible for me to compare the variations of the force with my determinations; I have however made use of the following method:— If X be the magnetism of the earth and m that of the needle, I seek not X but m; this latter quantity depends on the temperature t and the time T, as the needle is not constant; but if I combine all the values of m by an equation of the form

$$m=\mathrm{A}+\mathrm{B}\,e^{-a\mathrm{T}}+e\,.\,t$$

and calculate the constants, the error is about $\frac{1}{600}m$. Besides this, several simultaneous observations with Gauss's apparatus have shown that the value of m was itself correct.

The Inclinations have in part been determined by two needles which agreed very well with each other; they are so balanced that I can always take the mean of the eight arcs. On the other hand they are subject to the error of the axle, which I cannot exactly correct, but which does not however exceed 5′. It was only last summer, when I examined the subject more closely, that I became aware you had likewise the idea of loading the needle, and observing in different azimuths. In our latitudes the best loading is such as will cause the north pole to be in one set about 10° above, and in a second set 10° below the horizontal line. Three series which I made with one needle were calculated by my friend Claussen, who in doing so was led to a method of entirely eliminating the form of the axle. Take a well-balanced needle, the axles of which are not cylindrical; different degrees of magnetic force can be given to it without reversing the poles. Taking the strongest force as unity, it is not practically advantageous to go to lower ratios than $\frac{1}{4}$ or $\frac{1}{8}$. Though vibration experiments with dipping-needles are not generally advantageous, yet they suffice in this case, as an approximately correct proportion of the intensities is all that is wanted. It is sufficient to make, with each degree of intensity, the two observations with the face east and face west, without reversing the needle on its supports; if the latter is done, it gives a second determination, affording a check upon the first. You will then find that the mean of the two observations in one position of the axles is less than the

true inclination, and in the other position greater; the difference in both cases being more considerable as the intensity of the needle is weaker. Let I_0, I_1, I_2, &c. be the inclination observed with different intensities; T_0, T_1, T_2, &c. be the times of vibration, which increase as the index increases; a small correction is required, which can be determined in the following manner.—Take either I_0 or a somewhat less value (in round minutes) as being nearly correct, and let

$$I_0 - I_1 = \Delta I_1, ; I_0 - I_2 = \Delta I_2, \&c.,$$

then

$$\Delta I = x + T^2 y ;$$

x being the correction; thus I found

Az. 0°; I=70° 23′·8. Az. 180°; I=71° 26′·5. Mean 70° 55′·1. T=1s·167.
Az. 0; I=70 48·7. Az. 180; I=71 44·7. Mean 71 15·2. T=1·738.
Az. 0; I=66 16·0. Az. 180; I=84 16·5. Mean 75 36·3. T=4·25.

If I take 70° 55′·0 as nearly correct, I obtain the three following equations;

$$0'{\cdot}1 = x + (1{\cdot}167)^2 y \;;\; 20'{\cdot}2 = x + (1{\cdot}738)^2 y \;;\; 281{\cdot}3 = x + (4{\cdot}25)^2 y.$$

The three equations have not however the same weight, as the directive force is less in proportion as T is larger; in order to give them all the same weight I divide each by the coefficient of y, and thus obtain in logarithms

$$8{\cdot}86586 = 9{\cdot}86586\,x + y \;;\; 0{\cdot}82525 = 9{\cdot}51990\,x + y \;;$$
$$1{\cdot}19239 = 8{\cdot}74322\,x + y.$$

and hence $x = 21'{\cdot}8$; and the true dip=70° 33′·2.

I have here taken an imperfect needle, which I also observed in Azimuths of 30° to 30°; in one position of the axles I obtained 70° 39′·5; ±5′·9; and in a second 70° 42′·5; ±5′·4; mean 70° 41′·0. On a subsequent day I observed with a second needle and obtained 70° 43′·4; but an independent needle gave a dip 2′·6 greater, so that the two determinations are 70° 42′·1, 70° 42′·3, if we add to each the half difference.

In this method, in which no reversal is needed, the differences of the partial determinations will appear somewhat large, but you must not forget that instead of the ordinary eight observations only two have been taken.

I permit myself one additional remark. In observations on different azimuths, it is usual to take simply $\cot I = \cot I_1 \cos \alpha$; in latitudes where the dips are so high as here and in England, this equation may be employed without much error, as the force in azimuths perpendicular to the meridian is little less than in the meridian; but it is quite otherwise in small dips. With the decrease of force the possibility of error increases, and hence when the observations made in different azimuths are combined as by Kupffer, they have not the same weight. In more exact determinations I employ the following method.

Let K be the total, H the horizontal, V the vertical force, and a the nearly known azimuth; then

$$K \cos I = H \cos a; \ K \sin I = V; \ \tan I = \frac{V}{H} \cdot \frac{1}{\cos a};$$

whence
$$d\,I = \frac{\cos^2 I}{\cos a}\, d\left(\frac{V}{H}\right) + \frac{HV}{K^2} \sin a \,.\, d\,a.$$

If on the right we substitute for $\cos^2 I$ its value, then

$$d\,I = \frac{H^2 \cos a}{K^2}\, d\left(\frac{V}{H}\right) + \frac{HV}{K^2} \sin a\, d\,a.$$

As the possibility of error is inversely as the force, I multiply the equation by K, to give to the different determinations equal weight, thus

$$K\,d\,I = \frac{H^2 \cos a}{K}\, d\left(\frac{V}{H}\right) + \frac{HV}{K} \sin a\, d\,a:$$

having determined the dips in the customary manner with the approximately known values of a, I obtain the values d I, which serve to find $d\left(\frac{V}{H}\right)$; *i. e.* the correction of I.

I possess now with my instrument six needles, which I hope to compare very accurately with each other in the course of this year; but some months must first elapse, as I make all these determinations in the open air, and the bad autumn we have had has interrupted me in the work. I have had two of my needles fitted according to Fox's method, with wheels on their axles; two others have brass indexes, as was formerly proposed by Bernoulli and Euler (Berlin Trans. 1755), and I can now determine the absolute intensity with the inclinatorium. I know Fox's method only from a short notice in the London and Edinburgh Phil. Mag.; if I do not mistake, he proposed also to determine the declination by the same apparatus. With ordinary needles there remains an uncertainty. If we load the S. end of the needle so that the N. end is about 10° above the horizon, the S. end sinks down; and if we seek the azimuth in which the needle is perpendicular and then observe at about half a degree of azimuth on either side, the inclination alters so rapidly with the azimuth, that I have thus been even able to follow the diurnal variations of the declination; and the magnetic meridian may thus be determined for the observations of absolute declination whilst travelling.

I will not trouble you further as my letter is already so long, and will only add one request. The Phil. Trans. arrive here rather late, and the last communications which I have seen of yours contain Keely's determinations. All the observations of the Erebus and Terror have not yet appeared; in the Atlantic I know only the total intensities but without inclinations or declinations, and yet I am very anxious for some determinations that have been made between 10° and 20° of longitude in the higher latitudes to compare my calculations with them. If your time permits, I should be very much obliged

to you if you could communicate to me the inclination and force at some points. In the mean time I will occupy myself with the discussion of the two horizontal forces; unfortunately the number of determinations serving for this purpose is much smaller. For North America those recorded by Lamont in Dove's 'Repertorium' are for the most part in comparatively low latitudes.

May 1, 1851.

The EARL OF ROSSE, President, in the Chair.

A paper was read, entitled "An account of two cases in which an Ovule, or its remains, was discovered after death in the Fallopian tube of the unimpregnated human female, during the period of Menstruation." By H. Letheby, M.B. Communicated by W. B. Curling, Esq., F.R.S. Received Feb. 20, 1851.

At the commencement of the paper the author refers to the opinions of Drs. Power, Lee, Paterson, Barry, Girdwood, and Wharton Jones of this country, and also to those of MM. Valentin, Negrier, Pouchet, Gendrin, Raciborski, and Bischoff on the continent, respecting the supposed nature of the physiological phenomena manifested during the peirod of menstruation; and he mentions the law of Bischoff, namely, that "the ova formed in the ovaries of the females of all mammiferous animals, including the human female, undergo a periodical maturation and exclusion quite independently of the influence of the male seminal fluid. At these periods, known as those of 'heat' or 'the rut' in quadrupeds, and 'menstruation' in the human female, the ova which have become mature, disengage themselves from the ovary and are extruded. If the union of the sexes takes place at this period, the ovum is fecundated by the direct action of the semen upon it, but if no union of the sexes occurs, the ovum is nevertheless evolved from the ovary, and enters the Fallopian tube where it perishes." He states, however, that the arguments which have been advanced in support of this opinion in respect of the human female, are entirely of an analogical character; and that although the ovaries of women who have died during the menstrual period have been frequently examined, and Graafian follicles found in a recently ruptured state, yet the discovery of the liberated ovule had not, so far as the author was aware, ever been detected. The importance of his cases rests upon three grounds, namely,—1st, the circumstances under which the women had died; 2ndly, the finding of recently ruptured Graafian follicles; and 3rdly, the discovery of the ovule and its remains in the fluid matter of the Fallopian tubes.

In the first of the cases recorded, the woman died during a menstrual period. She had been an inmate of the London Hospital for twenty-four days before her death, where she was closely watched day and night by a nurse, in consequence of her having attempted

self-destruction by cutting her throat twenty-nine days before her death.

An examination of the body showed that the pelvic viscera were much congested; that the uterus was considerably enlarged; that the vagina contained a sero-sanguineous fluid; and that the hymen was unruptured. The ovaries were covered with stellate fissures, or cicatrices; and at one part of the left organ there was a purple spot having a ragged hole in its centre. By means of an incision into the gland through this spot, it was found that the opening led into a small cavity which was surrounded at its lower part by a dense tissue, infiltrated with dark coagulated blood (reference was here made to the preparation which shows the cavity and its coagulum). After macerating in spirit for a short time, it was noticed that the clot consisted of four parts, which the author described.

In other parts of the ovary several false *corpora lutea*, in different stages of decline, were found. The Fallopian tubes were highly congested, and the cavities of the tubes were filled with a bloody mucus. The left one contained at about one inch from its fimbriated end, a small vesicular body, which was, in the author's opinion, an ovule; for it consisted of nucleated cells and oil-globules. The fluid matters of the uterus and Fallopian tubes were made up of blood-discs, cylindrical epithelium, granular corpuscles, and a few spindle-shaped bodies.

The second case was that of a girl who had died at St. Luke's Hospital, where the supervision of the patient was quite as strict as that in the last case. In this instance the anatomical features were precisely like the preceding. The right Fallopian tube contained a globular body similar to that found in the left on the former occasion. This globular body, on being crushed between two pieces of glass and examined under the microscope, was found to consist externally of a mass of nucleated cells, the remains of the *tunica granulosa*, and of a transparent ring, enclosing an opake granular mass, and a highly pellucid spot. The author considered that this body was the liberated ovule, and the influence of chemical reagents served to support his opinion.

An examination of the *corpora lutea* found in both cases, showed that they consisted of large granular corpuscles and oil-globules.

The conclusions arrived at by the author were as follows:—

1. That ovules escape from the ovaries of women during the period of menstruation; and that their escape is a spontaneous act, taking place quite independently of sexual intercourse.

2. That immediately before, or else consentaneous with, the escape of an ovule, the whole substance of the Graafian follicle becomes charged with effused blood; and that a sort of fatty degeneration of the effused matter soon afterwards takes place.

3. That the mere presence of a yellow body containing a clot in the ovary, is not by any means a certain sign of recent impregnation.

4. That a sanguineous fluid is poured out over the whole mucous tract of the generative system during the catamenial period.

5. That the results of the observation tend to confirm the opinions

entertained by Wagner, Bischoff, Barry, and Wharton Jones, concerning the membranous nature of that portion of the ovule known as the *zona pellucida*.

6. That the oil-globules of the yelk are either enclosed in a distinct membrane, or else that a structureless solid material pervades the entire substance of the vitelline body, and so binds the several component elements of it together.

7. That the recognition of the germinal vesicle removes some doubts concerning its appearance and position in the germ-mass.

May 8, 1851.

The EARL OF ROSSE, President, in the Chair.

Professor Owen delivered the Croonian Lecture, being the substance of his paper "On the Megatherium."—Part II. Received May 6, 1851.

In his lecture the author premised a brief sketch of the successive steps which had led to the knowledge of the Megatherium acquired at the date of his researches, and of the different hypotheses which had been broached of its affinities, habits and food. He then recounted the mode of the acquisition of the complete skeleton, and of its articulation, at the British Museum, and commenced its description by the vertebræ of the trunk. These consist of 7 cervical, 16 dorsal, 3 lumbar, 5 sacral, and 18 caudal vertebræ. The first to the fifth dorsal vertebræ are characterized by having the ordinary number of articular processes (zygapophyses), two before and two behind; and by having three articular surfaces for the ribs on each side, one on the centrum, one on the neurapophysis, and one on the diapophysis. The sixth dorsal vertebra has an accessory zygapophysis between the posterior pair; the thirteenth dorsal has one between the anterior pair; the seventh to the twelfth inclusive have the accessory median zygapophysis between both the anterior and posterior pairs of the ordinary zygapophyses. The fourteenth and succeeding dorsals have no costal surface on the diapophysis or centrum. The fifteenth has both metapophysis and anapophysis—the latter with an articular surface: the sixteenth superadds the parapophysis with an articular facet.

The lumbar vertebræ lose the costal surface on the centrum, and retain the metapophyses, anapophyses and parapophyses. The nature of these accessory processes was explained by reference to the descriptions and figures of the exogenous processes of vertebræ in Part I. of the present Memoir.

The characteristics of the cervical vertebræ were next detailed.

Of the five anchylosed sacral vertebræ, three are confluent with the iliac bones, and two with the ischia.

The fourteen anterior caudals are characterized by articular surfaces for hæmapophyses. These elements are separate from each other in the first caudal, and confluent as usual at their distal ends, forming a 'chevron-bone' in the others. The posterior zygapophyses lose their articular surfaces in the eleventh caudal; the anterior

ones disappear in the twelfth: the metapophyses have subsided in the fifteenth. The neural canal is unclosed above in the sixteenth; and the vertebra is reduced to its central element in the last two caudals.

The skull is remarkable for its small proportional size, for its long and slender cranial portion, its large and complex zygomatic arches, its broad truncate facial part, with the slender produced premaxillaries, and for the great depth of the middle of the lower jaw.

The mastoid element developes a large tuberous process and a deep semicircular articular cavity for the stylohyal. The malar bone sends down a long process outside the lower jaw. The number of teeth is $\frac{5-5}{4-4}=18$, the fifth in the upper jaw being the smallest. They are alike in structure, and differ but little in shape: the grinding surface in most is crossed by two transverse ridges, the summits of which are formed by hard dentine; the rest of the tooth being composed of a central body of vaso-dentine and a peripheral mass of vascular cement. The microscopic characters of these several constituents of the teeth were then described. Each tooth is deeply implanted in the jaw, where it terminates without dividing into fangs, by a widely open pulp-cavity for a persistent matrix, ensuring perpetual growth. The stylohyal bone has the form of a hammer, with a long, slightly bent handle; one part of the head being thickened and rounded for articulation with the cavity in the mastoid.

The scapula presents almost the form of a trapezium, with the inferior angle bent outwards, increasing the depth of the subspinal fossa: there is a rudiment of a second spine, below the normal one: the acromion is expanded, produced and confluent with the coracoid; and the supraspinal fossa is perforated by a circular aperture. The clavicle has a well-marked sigmoid flexure, equally-developed obtuse extremities, without any articular surface. The humerus is remarkable for the enormous development of ridges for the attachment of the muscles, especially at its distal end: the inner condyle is not perforated as in the *Megalonyx*; it is devoid of a medullary cavity.

The ulna and radius are next described. The carpus consists of seven bones, three of which are proper to the first row, three to the second, and one is common to both: the latter answers to the 'scaphoides' and 'trapezium' in the human wrist, and articulates with the radius above, and the rudiment of the metacarpal of the pollex below.

Only four digits are developed, the first or 'pollex' being obsolete. The 'index' or second digit has three phalanges, the last supporting a large claw, and being twice as long as the two preceding phalanges. The proximal and middle phalanges of the 'digitus medius' are confluent. The ungual phalanx is shorter than that of the index, but has twice its vertical breadth. The metacarpals progressively increase in length from the first to the fifth. The fourth digit or 'annularis' has three phalanges, the last being unguiculate and longer than that of the 'medius.' The fifth digit has only two very short rounded phalanges, which were doubtless buried in a thick callous outer border of the foot, on which the Megatherium rested when applying the foot to the ground.

The pelvis shows the conversion of the ischiadic notch into a foramen by the anchylosis of the ischia with the posterior sacral vertebræ, and the union of the ossa pubis at a short anteriorly produced symphysis. The ilia are extraordinary for their vast breadth, and the thickness of the rugged labrum; indicative of the enormous muscular forces, of which this conspicuous part of the skeleton was the centre.

The femur is hardly less remarkable for its breadth and strength. The head is devoid of an impression for the ligamentum teres: but from the dimensions of the hemispheroid cavity receiving it, the author calculates that the muscles are aided in retaining the head of the femur in its place by an atmospheric pressure, with the barometer at 30 in., of not less than 660 pounds. At the distal end of the femur there is a great angular projection above the outer condyle. The rotular surface is continuous with that upon the outer condyle, but not with the inner one. The tibia and fibula are anchylosed together at both their extremities. Besides the patella in front of the knee-joint, there is a sesamoid 'poplitella' behind, wedged between the outer condyle and the tibia; which was doubtless imbedded at its base in the femoro-tibial articular capsule, and gave insertion to the tendon of the *popliteus* muscle. This sesamoid is not to be confounded with the 'fabella,' developed in many quadrupeds in the origin of the gastrocnemius, behind one or both condyles of the femur. The most peculiar feature in the tibia of the Megatherium is the form of the distal articular surface: especially the large and deep hemispherical excavation on the inner part of that surface for an unusually secure interlocking of the foot to the leg.

The bones of the tarsus are six in number in the Megatherium, and the astragalus offers corresponding peculiarities with those of the tibia with which it is articulated, and also remarkable modifications for the articulation of the naviculare and calcaneum. In the calcaneum, the length and strength of the hinder prominence forming the great lever for the extension of the foot, are amongst its most striking characteristics. These, with those of the other bones of the tarsus, are minutely detailed. There is no digit answering to the great toe or 'hallux,' nor any trace of the 'os cuneiforme' for that toe. The innermost of the 'ossa cuneiformia' answers to the middle one, and if any rudiment of the second toe ever existed independently, it has coalesced with that cuneiform bone: but this cannot be supposed to represent both middle and internal cuneiform bones and their digits blended together, as Cuvier supposed. There are no little bones missing from the inner side of the middle cuneiforme, as Pander and D'Alton conjectured. The first or innermost distinct metatarsal bone is that of the toe answering to the third, or digitus medius, in the pentadactyle foot: it is a short thick irregular wedge-shaped bone, with a large triangular concave base for the 'ecto-cuneiforme'; a semicircular flattened surface on the outer side for the fourth metatarsal, and a small semi-elliptic flat surface on the inner side for the 'meso-cuneiforme'. The distal end of the bone presents a strong median vertical obtuse ridge, dividing two

vertically elongated slightly concave surfaces, to which the anchylosed proximal and middle phalanges of the strong claw-bearing digit articulate. The ungual phalanx is shorter in proportion to its depth, than in the digitus medius of the fore-foot, and differs in the greater breadth of the upper part of the claw-sheath, and in the straighter cone, or bony core, which supported the claw. The metatarsals of the fourth and fifth toes are much larger than that of the third; but they support mere rudiments of digits reduced in each to two stunted phalanges, which were doubtless buried like those of the outer digit in the fore-foot in a kind of callous hoof.

Having completed the description of the skeleton, which is illustrated by an extensive series of accurate and highly finished drawings, the author proceeds to the comparison of the modifications of the osseous structure of the gigantic extinct animal with that in other known existing and extinct species of the class Mammalia.

The teeth agree in number, kind, mode of implantation and growth, with those of the Sloth, and their structure is a modification of that peculiar to the Sloth-tribe. All the modifications of the skull relating to the act of mastication, especially the large and complex malar bone, repeat the peculiarities presented by the existing Sloths. There are the same hemispheric depressions for the hyoid bone in the Megatherium as in the Sloth. In the number of cervical vertebræ the Megatherium, like the two-toed Sloth, agrees with the Mammalia generally. In the accessory articular surfaces afforded by the anapophyses and parapophyses of the hinder dorsal and lumbar vertebræ, the Megatherium resembles the Ant-eaters (*Myrmecophagæ*): but it does not resemble the Armadillos (*Dasypus*) in having long metapophyses, the peculiar development of which in those loricated *Bruta* has a direct relation to the support of their bony dermal armour. In the mesozygapophyses of the middle dorsal vertebræ the Megatherium is peculiar. In the small extent of the produced and pointed symphysis pubis it resembles the Sloths; and in the junction of both ilium and ischium with the sacrum, it manifests a character common to the Edentate order; but in the expanse and massiveness of the iliac bones, it can only be compared with other extinct members of its own peculiar family of Phyllophagous Edentata. Its habits necessitating a strong and powerful tail, we find this resembling in its bony structure that of other Edentata with a similar appendage, especially in the independency of the two hæmapophyses of the first caudal, a character which obtains in the Great Ant-eater and in some Armadillos; but this is no evidence of direct affinity to either of these families; the habits of the small arboreal Sloths render their eminently prehensile limbs sufficient for their required movements, and the tail is wanting. Had that appendage been proportionally as large as in the Megatherium, we cannot suppose that the caudal vertebræ would have materially differed from those of other Edentata.

In the coalescence of the anterior vertebral ribs with the bony sternal ribs, the Megatherium resembles the Sloths. This essential affinity is still more marked in the peculiarities of the scapula and

of the carpus. In the *Myrmecophaga jubata*, the scaphoid is distinct: in the *Manis* it coalesces with the lunare: in the *Dasypus gigas* the trapezoides is anchylosed to the second metacarpal: in the *Das. sexcinctus* it has coalesced with the trapezium. Not any of these characteristics are manifested by the Megatherium: its carpus repeats the peculiarities of that in the Sloths, viz. the reduction of the number of carpal bones to seven by the coalescence of the scaphoid with the trapezium. The first digit (pollex), which is retained in the Anteaters and Armadillos, is obsolete in the Megatherium as in the Sloths and Orycteropus: three digits are fully developed and armed with claws, as in the *Bradypus tridactylus*; and the fifth, though incomplete in the Megatherium, is better developed, because it was required in the ponderous terrestrial Sloth for its progression on level ground. In no existing ground-dwelling Edentate is the fifth digit deprived of its ungual phalanx, as in the Megatherium. The bones of the fore-foot of that extinct animal are thus seen to be modified mainly after the type of the *Bradypodidæ*.

The long bones of all the limbs are devoid of medullary cavities, as in the Sloths. The femur lacks the ligamentum teres as in the Sloths. The fibula is anchylosed to the tibia at both ends in Megatherium, as in *Dasypus*; but this is not the case in the closely-allied extinct Megatherioids called *Mylodon*, *Megalonyx* and *Scelidotherium*, a fact which diminishes the force of the argument which Cuvier deduced from the coalesced condition of the bones in the Megatherium in favour of its affinities to the Armadillos. The semi-inverted but firm interlocking articulation of the hind-foot to the leg shows the peculiarities of that joint in the Sloths exaggerated, and departs further from its characteristics in other Edentata. In all the existing *Edentata*, save the Sloths, the hind-foot is pentadactyle, and four of the toes have a long claw, even in the little arboreal *Myrmecophaga didactyla*: the departure by degradation from the pentadactyle type is a peculiar characteristic of the Sloth-tribe in the order. It is carried further in the same direction in the great extinct terrestrial Sloths. In these the mutilation of the foot has commenced on the outer side by the removal of the ungual phalanx from the fifth and fourth toes; but this accompanied by modifications which adapt these toes to the important office of support and progression of the body on level ground. In the scansorial Sloths, the three middle digits being equally developed for prehension, one toe on the outer and one on the inner side of the foot, are reduced to their metatarsal basis. In the Megatherium the mutilation of the foot on the inner side is carried to a greater extent; the innermost toe or hallux, with its entocuneiform bone, is wholly removed: the second toe is represented, like the first in the Sloths, by its cuneiform bone and a coalesced rudiment of the metatarse: and it is only the third toe or medius that repeats the condition of the claw-bearing toes in the climbing Sloths.

Finally, the author enters upon the question of the habits and food of the Megatherium. Guided by the general rule that animals having the same kind of dentition have the same kind of food, he

concludes that the Megatherium must have subsisted, like the Sloths, on the foliage of trees; but that the greater size and strength of the jaws and teeth, and the double-ridged grinding surface of the molars in the Megatherium, adapted it to bruise the smaller branches as well as the leaves, and thus to approximate its food to that of the Elephants and Mastodons. The existing Elephants and the Giraffe are specially modified to obtain their leafy food; the one being provided with a proboscis, and the entire frame of the lofty Giraffe adapting it to browse on branches above the reach of its largest ruminant congeners. If the Megatherium possessed, as Cuvier conjectured, a proboscis, it cannot, judging from the suborbital foramina, have exceeded in size that of the Tapir, and could only have operated upon branches brought near its mouth. Of the use of such a proboscis in obtaining nutritious roots, on the prevalent hypothesis that such formed the sustenance of the Megatherium, it is not easy to speculate: the hog's snout might be supposed to be more serviceable in obtaining those parts of vegetables; but no trace of the prænasal bone exists in the skull. A short proboscis would be very useful in rending off the branches of a tree prostrated and within reach of the low and broad-bodied Megatherium, and it would be aided in this act by the tongue, of which, both the hyoid skeleton, by its strength and articulation, and the foramina for the muscular nerves by their unusual area, attest the great size and power.

As regards the limbs, the Megatherium differs from the Giraffe and Elephant in the unguiculate character of certain of its toes, in the power of rotating the bones of the fore-arm, in the corresponding development of supinator and entocondyloid ridges in the humerus, and in the possession of complete clavicles. These bones are requisite to give due strength and stability to the shoulder-joint for varied actions of the fore-arm, as in grasping, climbing and burrowing: but they are not essential to scansorial or fossorial quadrupeds; the Bear and the Badger have not a trace of clavicles, and the mere rudiments of these bones exist in the Rabbit and the Fox. We must seek, therefore, in the other parts of the organization of the Megatherium, for a clew to the nature of the actions by which it obtained its food. In habitual burrowers the claws can be extended in the same plane as the palm, and they are broader than they are deep. In the Megatherium the depth of the claw-phalanx exceeds its breadth, especially in the large one of the middle finger; and they cannot be extended into a line with the metacarpus, but are more or less bent. Thus, although they might be used for occasional acts of scratching up the soil, they are better adapted for grasping; and the whole structure of the fore-foot militates against the hypothesis of Pander and D'Alton, that the Megatherium was a burrowing animal. The same structure equally shows that it was not, as Dr. Lund supposes, a scansorial quadruped; for, in the degree in which the foot departs from the structure of that of the existing Sloths, it is unfitted for climbing; and the outer digit is modified, after the ungulate type, for the exclusive office of supporting the body in ordinary terrestrial progression. It may be inferred from the diminished curvature and

length, and from the increased strength and the inequality of the claws, especially the disproportionately large size of that weapon of the middle digit, that the fore-foot of the Megatherium was occasionally applied by the short and strong fore-limb in the act of digging; but its analogy to that of the Ant-eaters teaches that the fossorial actions were limited to the removal of the surface-soil, in order to expose something there concealed, and not for the purpose of burrowing. Such an instrument would be equally effective in the disturbance of roots and ants; it is, however, still better adapted for grasping than for delving. But to whatever task the partially unguiculate hand of the Megatherium might have been applied, the bones of the wrist, fore-arm, arm and shoulder, attest the prodigious force which would be brought to bear upon its execution. The general organization of the anterior extremity of the Megatherium is incompatible with its being a strictly scansorial or exclusively fossorial animal, and its teeth and jaws decidedly negative the idea of its having fed upon insects; the two extremes in regard to the length of the jaws are presented by the phyllophagous and myrmecophagous members of the Edentate order, and the Megatherium in the shortness of its face agrees with the Sloths.

Proceeding then to other parts of the skeleton for the solution of the question as to how the Megatherium obtained its leafy food, the author remarks that the pelvis and hind limbs of the strictly burrowing animals, *e.g.* the Mole, are remarkably slender and feeble, and that they offer no notable development in the Rabbit, the Orycterope, or other less powerful excavators. In the climbing animals, as *e.g.* the Sloth and Orang, the hind-legs are much shorter than the fore-legs, and even in those Quadrumana in which the prehensile tail is superadded to the sacrum, the pelvis is not remarkable for its size or the expansion of the iliac bones. But in the Megatherium the extraordinary size and massive proportions of the pelvis and hind limbs arrest the attention of the least curious beholder, and become eminently suggestive to the physiologist of the peculiar powers and actions of the animal. The enormous pelvis was the centre whence muscular masses of unwonted force diverged to act upon the trunk, the tail, and the hind legs, and also by the 'latissimus dorsi' on the fore-limbs. The fore-foot being adapted for scratching as well as for grasping, may have been employed in removing the earth from the roots of the tree and detaching them from the soil. The fore-limbs being well adapted for grasping the trunk of a tree, the forces concentrated upon them from the broad posterior basis of the body may have co-operated with them in the labour, to which they are so amply adapted, of uprooting and prostrating the tree. To give due resistance and stability to the pelvis, the bones of the hind-legs are as extraordinarily developed, and the strong and powerful tail must have concurred with the two hind-legs in forming a tripod as a firm foundation for the massive pelvis, and affording adequate resistance to the forces acting from and upon that great osseous centre. The large processes and capacious spinal canal indicate the strength of the muscles which surrounded the tail,

and the vast mass of nervous fibre from which those muscles derived their energy. The natural co-adaptation of the articular surfaces shows that the ordinary inflection of the end of the tail was backwards as in a *cauda fulciens*, not forwards as in a *cauda prehensilis*. Dr. Lund's hypothesis, therefore, that the Megatherium was a climber and had a prehensile tail, is destroyed by the now known structure of that part.

But viewing, as the author conceives, the pelvis of the Megatherium as being the fixed centre towards which the fore-legs and fore-part of the body were drawn in the gigantic leaf-eater's efforts to uprend the tree that bore its sustenance, the colossal proportions of its hind extremities and tail lose all their anomaly, and appear in just harmony with the robust claviculate and unguiculate fore-limbs with which they combined their forces in the Herculean labour.

The author then referred to the *Mylodon robustus*, a smaller extinct species of the same natural family of phyllophagous *Bruta*, and to the additional arguments derivable from the skeleton of that animal in favour of the essential affinity of the Megatherium to the Sloths; and the light which the remarkable healed fractures of the skull of a specimen in the Museum of the College of Surgeons threw upon the habits and mode of life of the species.

Finally, with reference to the hypothesis of the German authors and artists of the degeneration of the ancient Megatherioids of South America into the modern Sloths, the author remarked that the general results of the labours of the anatomist in the restoration of extinct species, viewed in relation to their existing representatives of the different continents and islands, commonly suggested the idea that the races of animals had deteriorated in point of size. Thus the palmated Megaceros is contrasted with the Fallow-deer, and the great Cave-bear with the actual Brown Bear of Europe. The huge Diprotodon and Nototherium afford a similar contrast with the Kangaroos of Australia, and the towering Dinornis and Palapteryx with the small Apteryx of New Zealand. But the comparatively diminutive aboriginal animals of South America, Australia and New Zealand, which are the nearest allies of the gigantic extinct species respectively characteristic of such tracts of dry land, are specifically distinct, and usually by characters so well marked as to require a subgeneric division, and such as no known or conceivable outward influences could have progressively transmuted. Moreover, as in England, for example, our Moles, Water-voles, Weasels, Foxes and Badgers, are of the same species as those that co-existed with the Mammoth, Tichorrine Rhinoceros, Cave Hyæna, Bear, &c.; so likewise the remains of small Sloths and Armadillos are found associated with the Megatherium and Glyptodon in South America; the fossil remains of ordinary Kangaroos and Wombats occur together with those of gigantic herbivorous marsupials; and there is similar evidence that the Apteryx existed with the Dinornis: and the author offered the following suggestions as more applicable to or explanatory of the phenomena than the theory of transmutation and degradation. He observed, that in proportion to the bulk of an animal is the difficulty

of the contest which, as a living being, it has to maintain against the surrounding influences which are ever tending to dissolve the vital bond and subjugate the organised matter to the ordinary chemical and physical forces. Any changes, therefore, in the external circumstances in which a species may have been created to exist, will militate against that existence in probably a geometrical ratio to the bulk of such species. If a dry season be gradually prolonged, the large mammal will suffer from the drought sooner than the small one; if such alteration of climate affect the quantity of vegetable food, the bulky Herbivore will first feel the effects of the stinted nourishment; if new enemies are introduced, the large and conspicuous quadruped or bird will fall a prey, whilst the smaller species might conceal themselves and escape. Smaller quadrupeds are usually, also, more prolific than larger ones. The actual presence therefore of small species of animals in countries where the larger species of the same natural families formerly existed, is not to be ascribed to any gradual diminution of the size of such larger animals, but is the result of circumstances which may be illustrated by the fable of 'the oak and the reed'; the small animals have bent and accommodated themselves to changes under which the larger species have succumbed.

May 15, 1851.

The EARL OF ROSSE, President, in the Chair.

The following papers were read:—

1. "Note relating to M. Foucault's new mechanical proof of the Rotation of the Earth." By C. Wheatstone, Esq., F.R.S., Corresponding Member of the Academies of Science of Paris, Berlin, Brussels, Turin, Rome, Dublin, &c. Received May 15, 1851.

The experiment which led M. Foucault to his ingenious and interesting researches relating to the rotation of the earth, is stated by him thus:—"Having fixed on the arbor of a lathe and in the direction of the axis, a round and flexible steel rod, it was put in vibration by deflecting it from its position of equilibrium and leaving it to itself. A plane of oscillation is thus determined, which, from the persistence of the visual impressions, is clearly delineated in space; now it was remarked that, on turning by the hand the arbor which serves as a support to this vibrating rod, the plane of oscillation is not carried with it."

This persistence of the plane of oscillation of a vibrating rod, notwithstanding the rotation of the point to which its end is fixed, does not appear to have hitherto been made the subject of philosophical observation. Ordinary notions even seem to have been opposed to this now recognised fact. Chladni in his treatise on Acoustics, in the chapter "On the co-existence of vibrations with other kinds of motion," states as follows:—

"Vibratory motions may co-exist with all other kinds of motions

in an infinity of different manners, as has been demonstrated by Dan. Bernoulli and L. Euler in vols. xv. and xix. of the *Nov. Comment. Acad. Petrop.*, and confirmed by experiment. These co-existences of different motions occur in all sonorous bodies without exception: we may, for example, produce the sound of a string stretched on a board, or that of a plate, a tuning-fork, a bell, &c.; and while the vibrations still last, impress on this sonorous body a motion of rotation round its axis, and at the same time a progressive motion: thus all these motions may be performed in the same time, without one being hindered by the other; but the absolute motion of each point will be very complicated."

Now this is true only when the vibrating body is constrained to vibrate in one direction. When the rod or string is equally flexible in every direction, the plane of vibration given to it from any original impulse is constantly maintained whatever may be the velocity of rotation communicated to its point of support, provided the axis of vibration remains in the same position, or moves only parallel to itself.

This observed independence of the plane of oscillation on the point of attachment led M. Foucault to assume, that were a flexible pendulum suspended from a fixed point in the prolongation of the axis of the earth, that is above one of the poles, and maintained constantly in vibration, the plane of oscillation maintaining an invariable position in space would appear to a spectator on the earth's surface and moving with it to make an entire revolution in twenty-four hours, but in the opposite direction to that of the rotation of the earth.

What takes place at other points of the earth's surface is more difficult to determine; but M. Foucault, from mechanical and geometrical considerations, was led to the conclusion that the angular displacement of the plane of oscillation is equal but opposite to the angular motion of the earth multiplied by the sine of the latitude. According to the theory of rotation, first established by Frisi and more fully developed by Euler and Poinsot, the velocity of rotation of the earth may be considered as the resultant of two angular velocities, one round the vertical of the point where the observer is placed, and the other round the meridian or horizontal line lying N. and S. The component of the angular velocity estimated round the vertical axis is $n \sin \gamma$, and the plane of oscillation not participating in this motion remains at rest with respect to it, and therefore appears to an observer moving with the point, to rotate with the same velocity in the contrary direction.

The experiment made by M. Foucault is said, both in the direction and magnitude of the motion of the plane of oscillation of the pendulum, fully to confirm the indications of theory. The difficulty, however, of the mathematical investigation of the subject, and the delicacy of the experiment, liable as it is to so many extraneous causes of error, have induced many persons to doubt either the reality of the phænomenon or the satisfactoriness of the explanation. Another experimental proof, therefore, not depending on the rotation

of the earth, that the plane of oscillation of a vibrating line remains at rest with relation to the vertical component of the real axis of rotation, may not be unacceptable. With this view I have devised the apparatus I am about to describe.

A semicircular arch from one to two feet radius is fixed vertically on a horizontal wheel, and may thus be moved with any degree of rapidity from any one azimuth to another. A rider slides along the inner edge of the arch, which is graduated, and may be fixed at any degree marked thereon. A spiral spring wire, by means of which a slow vibration is obtained with a comparatively short length, is attached at the lower end to a pin fixed in the axis of the semicircle, so that the point of attachment may be in the axis of rotation, and at the upper end it is fixed to a similar pin in a parallel position fixed to the rider. The vertical semicircle is not placed in a diameter of the horizontal wheel, but parallel to it, at such distance as not to intercept, from the eye of the observer, the vertical plane passing through the diameter, and in which plane the wire in all its positions remains.

When the upper end of the wire is placed at 90°, that is when it coincides with the axis of rotation, if the wire be caused to vibrate in any given plane, say from N. to S., it will continue to do so whatever rotation may be communicated to the wheel; so that with respect to the moving wheel, or the axis of the wire, the plane of vibration will move with the same velocity and in the opposite direction. When the rider is fixed at 30°, and the wire makes therefore an angle of 60° with the axis of rotation so as to describe in its motion the surface of a cone having this inclination to the vertical, it will be observed that the plane of vibration makes one complete rotation during two rotations of the wheel: this is best observed by fixing the eye so that its axis shall coincide with a line in the same vertical plane with the wire, while walking round with the wheel during its rotation. When the rider is fixed at $19\frac{1}{2}$°, the plane of vibration makes one rotation during three rotations of the wheel; when fixed at $14\frac{1}{2}$°, it makes one rotation during four of the wheel, &c.; and when it is fixed at 0°, the wire lying horizontally, no rotation of the plane of vibration occurs. It is needless to observe that the sines of 90°, 30°, $19\frac{1}{2}$°, $14\frac{1}{2}$°, 0°, correspond to the numbers 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, 0, the reciprocals of the numbers expressing the respective times of rotation*.

It is not necessary that the wire should have one of its ends fixed in the axis of rotation: if it be parallel to a wire so fixed, the rotation of the plane of vibration will be exactly similar; in such case the wire

* When the dimensions of the apparatus are as above given, I find that hardened brass wire (No. 26), coiled so as to form a helix of one-quarter of an inch in diameter, shows the effect well. The thickest spiral wire employed in the manufacture of artificial flowers, which can be procured of any wire-drawer, will also answer the purpose.

The best way of setting the wire in vibration is to press the finger upon it in the middle, so as to deflect it in the plane in which the vibrations are required to continue, and then suddenly to withdraw the finger in the direction of the vibrations. The deflection must not be too great, or the elasticity of the wire will be injured.

or axis of vibration will describe the surface of two cones having their common apex in the axis of rotation.

The axis of a flexible pendulum can only assume a position vertical to the point of the earth's surface over which it is placed. Were it possible to maintain the vibration of a stretched wire occasioned by an original impulse, for a sufficient length of time, the apparent rotation of its plane of vibration would vary with the inclination of the wire to the axis of the earth: placed in this axis, it would make a rotation in 24 hours, it would become progressively slower according to the law above given, as it approaches the plane of the equator, and when anywhere in this plane the vibrations would always be performed in the same direction.

2. "Report of further Observations made upon the Tidal Streams of the English Channel and German Ocean, under the authority of the Admiralty, in 1849 and 1850." By Captain F. W. Beechey, R.N. Communicated by G. B. Airy, Esq., F.R.S. &c., Astronomer Royal. Received March 27, 1851.

This is the continuation of a report which the author made upon the tidal phenomena of the Irish Sea and English Channel in 1848. After detailing the manner in which the investigation had been conducted, and the great care which had been bestowed upon the observations, which are numerous, the author enters into an explanation of the whole system of tidal streams in the English Channel and North Sea, as deduced from these observations, and also as to what he considers to be the cause of the peculiar movement of the streams in these channels. He supposes, in conformity with Dr. Whewell's theory, a tide-wave to pass along the western shores of Europe, and to enter the English Channel and North Sea by opposite routes, and to arrive off the Texel and Lynn at the same tidal hour as the tide-wave in the English Channel arrives off the Start and Jersey. From these points there are thrown off branch or derivative waves, which differ materially both in dimensions and rate of travelling from the parent wave. These waves roll on towards the strait of Dover and there merge into each other and form a combined wave.

The effect of this wave upon the tidal establishments of the Channel had long been known; but its influence upon the streams of the Channel had never before been considered, nor had any observations upon them been systematically undertaken.

In arranging the plan of observation the author considered that, as the combined wave was common to both the English Channel and North Sea, the tidal streams of both these channels would be found to correspond in every important particular, and that the movement of the streams throughout the strait would be materially influenced, if not wholly governed, by the motion of the combined wave; that the time of this wave attaining its greatest altitude would thus afford a standard to which the turn of the streams throughout the Channel might be advantageously referred; and that there would be found in this Channel, as in the Irish Sea, which is equally under the influence of a combined wave, a stream which would turn nearly

simultaneously throughout the strait with the times of high and low water on the shore at the point of combination or virtual head of the tide.

Accordingly the observations were conducted upon this plan, and all the movements of the stream were referred to the time of high water at Dover, which had been determined upon as the standard from its being situated nearly at the point where the combined wave is formed. It appeared from the intervals which this mode of comparison afforded, that whilst the water was *rising* at Dover, the stream of the channels on both sides ran *towards* that place; and on the contrary, in the *opposite* direction whilst the water was *falling* there; and that these streams pursued a steady course throughout the tide, and extended from a line joining the Texel and Lynn, in the North Sea, to a line joining the Start and Jersey in the English Channel. Beyond these limits the streams of the Channel were found to encounter those of the offing or parent wave, and to occasion the tides in those localities to partake of a rotatory character, revolving for the most part with the sun, and having scarcely any interval of slack water.

The line of meeting of these streams was found not to be a stationary line, neither in those parts where the Channel-stream encounters the offing stream, nor where the streams meet in the strait of Dover, but was found to shift from west to east as the tide rises and falls at Dover, beginning at Beachy Head and ending at the North Foreland; so that the space occupied by the Channel-stream always preserves the same dimensions, notwithstanding its limits extend over a distance of 360 miles. The strait of Dover was found never to have slack water throughout its whole extent at any time, as was the case in the other ports of the Channel, from which it differs in this respect; and the streams in this locality have in consequence been designated as those of the "Intermediate tide."

As the simultaneous turn of the stream throughout the Channel is a point of considerable interest and entirely new, the author takes considerable pains to point out the methods by which this important fact was ascertained, and refers to the observations kept on board the light vessels along the coast, and to others made at various important stations; and whenever any contradictory evidence appears, the cause of the discrepancy is inquired into and explained. It was found, for instance, that in a port of the North Sea, near the node referred to by Dr. Whewell, there was a retardation of an hour in the turn of the stream; and, upon an investigation as to the cause of this delay, it is seen to be owing to the stream running round the Texel and entering the North Sea at a time when the Channel-stream had ceased; but as soon as the Channel-stream acquired sufficient strength, it speedily drove the Texel stream back and confined it to its proper limits. In the English Channel also a similar discrepancy is observable near the coast of France; but this also the author considers to be fully accounted for by causes incidental to that part of the Channel, and not to be of sufficient con-

sequence to derogate from the character ascribed to the general motion of the water throughout the strait.

A reference is made to the erroneous opinions which have hitherto been entertained with respect to the motion of the streams of our channels; and the author concludes his paper by explaining his views as to the manner in which the turn of the stream is rendered simultaneous by the rapid rise of the combined wave in the centre of the strait, and expresses a hope that he has satisfactorily shown from the observations, that throughout the English Channel and North Sea the movement of the stream may safely be referred to a common standard. This, it is considered, will be of great importance to navigation; as thus the seaman's progress through these moving waters will be freed from the numerous and perplexing references he was before obliged to make, and which too often—and, it is to be feared, in many instances too fatally—caused the tides to be wholly disregarded. All uncertainty as to the effect of the stream will henceforward, it is expected, be obviated by a simple reference to a tide table.

The paper, which is accompanied by numerous plans and charts, forms a practical illustration of the tidal streams of straits, under the influence of a combined wave.

May 22, 1851.

The EARL OF ROSSE, President, in the Chair.

Edward Schunck, Esq. was admitted into the Society.

Mr. James Smith gave notice, that at the next meeting of the Society he would propose His Grace the Duke of Argyll for immediate ballot, to which as a peer of the realm His Grace is entitled.

The following papers were read:—

1. "Additional Observations on the Diffusion of Liquids." By Thomas Graham, Esq, F.R.S., F.C.S. &c. Received March 27, 1851.

The experiments detailed in this paper were conducted with the same apparatus and in the same manner as those described in the author's two former papers on this subject. The diffusion was generally made from four different proportions of each solution, so as to exhibit pretty fully the character of the salt in reference to this property. The salts operated upon were of two bases only, potash and soda, but the acids were considerably varied, so as to include the hydrates, carbonates, sulphates, sulphites, hyposulphites, sulphovinates, oxalates, acetates and tartrates of these bases.

The times chosen for the corresponding potash and soda salts, with the view of obtaining equal diffusions, were always in the proportion of 1·4142 to 1·7320, that is, as the square root of 2 to the square root of 3. Eight cells were diffused of the 1 and 2 per cent. solutions, and four cells of the 4 and 8 per cent. solutions. The salts were always taken anhydrous.

Diffusion of hydrate of potash in 4·04 days at 63°·4: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·56	1·022
„ 2 „ „	12·84	2·
„ 4 „ „	25·04	3·900
„ 8 „ „	52·24	8·137

Diffusion of hydrate of soda in 4·937 days at 63°·3: two cells.

	Grs.	Ratio.
From 1 per cent. solution	5·81	1·048
„ 2 „ „	11·09	2·
„ 4 „ „	20·86	3·765
„ 8 „ „	40·44	7·30

The nearest approach to equality of diffusion in the hydrates of potash and soda is exhibited by the 1 per cent. solutions, which are as 6·56 to 5·81, or as 100 to 88·57.

Diffusion of carbonate of potash in 8·083 days, double the time of hydrate of potash, at 63°·7: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·13	1·028
„ 2 „ „	11·92	2·
„ 4 „ „	22·88	3·839
„ 8 „ „	45·44	7·624

Diffusion of carbonate of soda in 9·875 days, double the time of hydrate of soda, at 63°·4: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·02	1·028
„ 2 „ „	11·70	2·
„ 4 „ „	21·42	3·661
„ 8 „ „	39·74	6·792

The diffusion of carbonate of potash appears similar to that of the carbonate of soda in the 1 and 2 per cent. solutions, but they diverge in the 4 and 8 per cent. solutions.

Diffusion of sulphate of potash in 8·083 days at 60°·3: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·16	1·062
„ 2 „ „	11·60	2·
„ 4 „ „	22·70	3·914
„ 8 „ „	43·92	7·572

Diffusion of sulphate of soda in 9·875 days at 59°·9: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·33	1·055
„ 2 „ „	12·00	2·
„ 4 „ „	21·96	3·66
„ 8 „ „	41·38	6·896

The following results of the diffusion of various salts from the 2 per cent. solution were obtained; the times being always 8·083 days for the potash salts and 9·875 days for the soda salts:—

	Grs.	Ratio.
Sulphate of potash at 60°·3 (as above)...	11·60	100·
Sulphite of potash at 59°·5	11·63	100·26
Sulphite of soda at 59°·6	11·83	101·72
Hyposulphite of potash at 59°·8	12·37	106·44
Hyposulphite of soda at 59°·9	11·89	101·10
Sulphovinate of potash at 59°·8	12·60	108·62
Sulphovinate of soda at 59°·6	13·03	
Tartrate of potash at 59°·9	10·96	
Tartrate of soda at 59°·6	10·65	
Carbonate of potash at 59°·9	10·73	
Carbonate of soda at 59°·6	10·65	

Diffusion of oxalate of potash in 8·083 days at 59°·9: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·20	1·019
„ 2 „ „	12·17	2·
„ 4 „ „	23·04	3·789
„ 8 „ „	42·82	7·042

The oxalate of potash corresponds closely with sulphate of potash.

The diffusion from the 1 per cent. solution of oxalate of soda at 59°·9, gave 6·24 grs. in 9·875 days, or a nearly equal diffusate to that of the oxalate of potash.

Diffusion of acetate of potash in 8·08 days at 60°·3: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·44	1·028
„ 2 „ „	12·52	2·
„ 4 „ „	23·44	3·744
„ 8 „ „	47·26	7·549

The acetate is found to exceed sensibly the sulphate and oxalate of potash in diffusibility, at the preceding temperature.

Diffusion of acetate of soda in 9·87 days at 59°·6: two cells.

	Grs.	Ratio.
From 1 per cent. solution	6·67	1·070
„ 2 „ „	12·46	2·
„ 4 „ „	25·04	4·019
„ 8 „ „	48·04	7·711

The diffusion of acetate of soda presents a general parallelism to that of acetate of potash for the times chosen, the temperatures of the two series of experiments differing only 0·7 degree.

The salts of potash and soda of the same base appear therefore to give approximately equal amounts of diffusate, when diffused for the two different times chosen, with the remarkable exception of the hydrates of these two bases. The relation is most observable in the small proportions of salt, or weak solutions, such as the 1 and 2 per cent. solutions. The soda salts, as a general rule, fall off in diffusibility in the higher proportions as compared with potash salts.

The double tartrate of potash and soda was observed to undergo decomposition in diffusion, the tartrate of potash separating from the tartrate of soda from the higher diffusibility of the former.

2. "On the Annual Variation of the Magnetic Declination, at different periods of the Day." By Lieut.-Col. Sabine, R.A., V.P. and Treas. R.S. &c. Received April 30, 1851.

In this communication the author has arranged and presented together the Annual variations which the magnetic declination undergoes at every hour of the day at the four Colonial Observatories established by the British government, at Toronto, Hobarton, the Cape of Good Hope and St. Helena. This has been done by means of a graphical representation, in which the annual variations at every hour are shown by vertical lines varying in length according to the amount of the range of the annual variation at each hour; each line having also small cross lines marking the mean positions of the several months in the annual range. The mean declination in the year at the respective hours is marked by a horizontal line which crosses all the verticals at each station. The hours are those of mean solar time at each station, the day commencing at noon. The annual variations represented in the plate were obtained at Toronto from three years of observation, viz. 1845, 46, 47; at Hobarton from five years, viz. July 2nd, 1843 to July 1, 1848; at the Cape of Good Hope from five years, viz. July 2nd, 1841 to July 1st, 1846; and at St. Helena from three years, viz. July 2nd, 1844 to July 1st, 1847.

The author observes that it is perceived at the first glance at the plate, that the range of variation at all the four stations is considerably greater during the hours of the day than during those of the night; and that there is a great similarity, though not a perfect identity, at all the stations in the relative amount of the range at different hours. Further, that the amount does not progressively enlarge to a maximum at or about noon, when the sun's altitude is greatest; or at the early hours of the afternoon, when the temperature is greatest; but that at all the stations the increase of the range is most rapid in the first or second hour after sunrise; and that its extent at the hours from 7 to 9 A.M. is not exceeded at any subsequent hour at Hobarton, the Cape and St. Helena, whilst at Toronto the great enlargement takes place even earlier, the hours of 6, 7 and 8 A.M. being exceeded by none, though they are equalled by a second increase at noon and the two following hours. This second enlargement is perceptible at the same hours at Hobarton and St. Helena.

With reference to the relative positions of the several months in each of the vertical lines, or at the different hours, it is observed that certain months, which are found congregated at the one extremity of the range during the early hours of the morning, undergo a transfer towards the opposite extremity at a subsequent period of the day; thus the months June, July, August usually occupy one extremity of the range, and November, December, January the other extremity, in the morning hours, and until from 8^h to 10^h A.M., when each of the two groups is respectively transferred towards the opposite extremity to that which it previously occupied. The period at which this transfer takes place is somewhat

earlier at Toronto and St. Helena than at the Cape of Good Hope and Hobarton. The comportment of the two equinoctial months, March and September, at the Cape of Good Hope and St. Helena is pointed out as presenting a remarkable contrast to that of the two solstitial groups which have been described, and at the same time a still more remarkable contrast to each other, March being at almost all the hours on the West, and September on the East, of the mean line.

In conclusion the author points out one or two practical considerations suggested by the facts under notice :—

1. That as, in the Annual Variation represented in the plate, the same months occupy positions on opposite sides of the mean line at different parts of the twenty-four hours, the *mean* annual variation, or that which is shown by the mean values in each month taken from *all* the observation hours, must be merely a residual and not an absolute quantity; and that consequently natural features must be more or less masked in deductions in which only mean values are brought into view. In fact, as has been shown in the published volumes of the observations at St. Helena and Hobarton, the mean annual variation at those stations is so small as to be scarcely sensible. But when we resolve these mean results into their respective constituents, viz. the annual variation *at each of the observation hours*, there is then at once disclosed to us an order of natural phenomena, very far from inconsiderable in amount, systematic in general aspect, and apparently well deserving the attention of those who are occupied in the delightful and highly intellectual pursuit of tracing the agencies of nature.

2. We perceive in the variations of the position of the several months in the annual range, the necessity of paying regard to the period of the year, as well as to the period of the day at which observations have been made which do not include long intervals, and from which, nevertheless, inferences are drawn in respect to secular change. Such observations, when not those of a fixed observatory, are usually made at some hour in the day-time, when it needs only a glance at the plate to perceive that annual as well as diurnal variation-corrections are required, unless the month as well as the hour are the same in the earlier and later observations. A table of corrections for every hour of the day to the mean value in each month—corrections derived, as in the instances now before the Society, from a series of strictly comparable observations continued for several years—should be considered, not merely as a desirable, but as an almost indispensable provision, in countries where magnetic surveys are conducted with the degree of perfection of which they are now susceptible.

3. The subjoined Notice by Monsieur A. Quetelet, Foreign Member of the Society, is entitled "Sur les ondes atmosphériques."

J'ai eu l'honneur de déposer, dans la séance précédente, un travail imprimé sur les *pressions et ondes atmosphériques*, fesant partie d'un ouvrage sur le climat de la Belgique. Si je me permets d'appeler,

aujourd'hui, l'attention de l'assemblée sur quelques-uns des principaux résultats auxquels je suis parvenu, c'est bien moins pour établir ce qui a été fait, que pour signaler les lacunes qui existent encore dans cette branche importante de la physique du globe, et pour inviter les observateurs à les combler.

Dans les fluctuations que subissent les pressions atmosphériques, on sait qu'un même *minimum* se présente en général sur une série de points qui sont liés par la loi de continuité, et qui forment ainsi, à la surface du globe, une ligne plus ou moins étendue. Cette ligne de pression *minimum* est mobile et se déplace avec des vitesses et des directions que je me suis principalement proposé d'étudier. On peut, par analogie avec ce qui se passe sur les mers, nommer *onde atmosphérique*, l'intervalle qui sépare deux lignes de pression minimum; et *crête de l'onde* la ligne de pression maximum.

Sir John Herschel a été le premier à appeler sérieusement l'attention des physiciens sur la nature des ondes atmosphériques; et il est parvenu à plusieurs résultats intéressants. M. Birt s'est principalement attaché à signaler les propriétés de ce qu'il nomme la grande onde de Novembre.

En mettant à profit toutes les observations régulières que j'ai pu réunir pour le nord de l'Europe et de l'Asie, j'ai essayé d'embrasser la solution du problème dans toute sa généralité. On pourra voir, dans la mémoire que j'ai eu l'honneur d'offrir à la Société Royale, les ondes atmosphériques représentées sur une série de cartes figuratives pour trois mois consécutifs. Ces cartes montrent surtout que—

1. L'atmosphère est généralement traversée par plusieurs systèmes d'ondes différents; mais, au milieu de tous les mouvements particuliers, il se prononce un système d'ondes prédominant qui semble rester à peu près constant pour un même climat.

2. Les ondes atmosphériques, tant en Europe qu'en Asie, se propagent du nord au sud, sans avoir toutefois la même vitesse; elles marchent plus rapidement dans le système Asiatique et dans le système de l'Europe centrale, qu'en Russie ou dans les montagnes de l'Oural.

3. Les ondes atmosphériques semblent se propager avec moins d'obstacles à la surface des mers qu'à l'intérieur des terres. En général, les aspérités du globe, et particulièrement les chaînes de montagnes, diminuent leur vitesse et modifient aussi leur intensité.

4. La vitesse avec laquelle les ondes atmosphériques se propagent est très variable; elle peut être estimée moyennement de 6 à 10 lieues de France à l'heure: elle est un peu plus grande dans l'Europe centrale, et moindre en Russie.

5. Les directions des vents n'ont pas de rapports apparents avec les directions des ondes barométriques; il parait en être de ces ondes comme des ondes sonores qui se transmettent dans toutes les directions, malgré l'obstacle des vents, lesquels pourraient à la vérité, en modifiér l'intensité et la vitesse.

Ces premiers résultats, soumis à de nouveaux examens, et controlés par des systèmes d'observations plus complets, conduiront certainement à des conclusions très importantes pour la météorologie et

l'étude des grands phénomènes de la physique du globe. On peut reconnaître, dès à présent, que le nombre des stations nécessaires pour établir la loi de continuité est insuffisant, surtout dans toute l'étendue de l'Asie.

June 5, 1851.

The EARL OF ROSSE, President, in the Chair.

The Annual Meeting for the election of Fellows was held this day.

The Statutes for the election of Fellows having been read, Dr. Roget and Mr. Spence were, with the consent of the Society, appointed Scrutators.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected:—

Charles Cardale Babington, Esq.	Augustus William Hofmann, Esq.
Thomas Snow Beck, M.D.	Thomas Henry Huxley, Esq.
Charles James Fox Bunbury, Esq.	William Edmond Logan, Esq.
George T. Doo, Esq.	James Paget, Esq.
Edward B. Eastwick, Esq.	George Gabriel Stokes, Esq.
Captain Charles M. Elliot.	William Thomson, Esq.
Captain Robert FitzRoy, R.N.	Augustus V. Waller, M.D.
John Russell Hind, Esq.	

The Society then adjourned to the 19th of June.

June 19, 1851.

The EARL OF ROSSE, President, in the Chair.

The following gentlemen were admitted into the Society:—

Charles Cardale Babington, Esq.	Captain Robert Fitz-Roy, R.N.
Thomas Snow Beck, M.D.	Thomas Henry Huxley, Esq.
Edward B. Eastwick, Esq.	William Edmond Logan, Esq.
Captain Charles M. Elliot.	George Gabriel Stokes, Esq.

His Grace the Duke of Argyll was balloted for, and elected a Fellow of the Society.

The following papers were then read:—

1. "Researches in Symbolical Physics. On the Translation of a Directed Magnitude as Symbolised by a Product. The Principles of Statics established symbolically." By the Rev. M. O'Brien, M.A., late Fellow of Caius College, Cambridge, and Professor of Natural Philosophy and Astronomy in King's College, London. Communicated by W. A. Miller, M.D., F.R.S. &c. Received April 10, 1851.

In this communication the author (starting from the well-known

theorem, that two sides of a triangle are equivalent to the third, when *direction*, as well as magnitude, is taken into account) proposes an elementary step in symbolization which consists in representing the *Translation of a Directed Magnitude* by a *Product*. Any magnitude which is drawn or points in a particular direction, such as a force, a velocity, a displacement, or any of those geometrical or physical quantities which we exhibit on paper by *arrows*, he calls a *directed magnitude*. By the *translation* of such a magnitude he means the removal of it from one position in space to another *without change of direction*.

U representing any directed magnitude and u any distance, the translation of U to any parallel position in space, in such wise that every point or element of U is caused to describe the distance u, is termed the *translation of* U *along* u.

This translation consists generally of two distinct changes, one the *lateral* shifting of the line of direction of U, and the other the motion of U *along* its line of direction. The former is called the *transverse effect*, the latter the *longitudinal effect* of the translation of U along u.

Both these effects are shown to be *products* of U and u; the transverse effect is represented by uU, and the longitudinal by u.U, inserting a dot between the factors in the latter for the sake of distinction.

The author then goes on to apply the principles established to the proof of the *Parallelogram of Forces*, and the determination of the effect of any set of forces on a rigid body. In doing this a remarkable symbolization of the *point of application*, as well as the direction and magnitude of a force, is obtained, namely, that the expression $(1+u)$U represents a force U acting at a distance u from the origin.

The principles of statics are deduced with remarkable facility from the symbolical representation of the translation of a force along a given distance.

2. "On an Air-Engine." By James Prescott Joule, F.R.S. &c. Received May 13, 1851.

The air-engine described in this paper consists of a pump by which air is compressed into a heated receiver; and a cylinder, through which the air passes again into the atmosphere. The difference between the work evolved by the cylinder and that absorbed by the pump, constitutes the work evolved by the engine on the whole. Two tables are given; the first of which contains the pressure, temperature and work absorbed, for various stages of the compression of a given volume of air. The second table gives the theoretical duty of the air-engine described, worked at various pressures and temperatures. The temperature recommended to be adopted in practice is as little below the red heat as possible, which would involve the consumption of only about one-third the amount of fuel consumed by the best steam-engines at present constructed.

3. "Experiments made at York (Lat. 53° 58′ N.) on the Deviation of the Plane of Vibration of a Pendulum from the meridional and other vertical planes." By John Phillips, Esq., F.R.S. Received June 3, 1851.

The following is the author's account of these experiments.

The experiments, of which the following is a notice, were made partly in the north-western Tower of the Minster, and partly in a room of my residence. The latter attempts have only within a few days acquired sufficient method and consistency to deserve reporting; nor have the trials in the Minster been uniformly successful.

Mr. Thomas Cooke, an able optician of York, began the experiments in the Minster. On the 30th of April, Mr. Gray and myself observed the vibration of his pendulum, and found it so accurate as to justify the belief that it might not only indicate the direction, but measure the angular value of the deviation of the pendulum plane from the meridian. Computing this value for an hour to be 12°+, we watched the result and found the arc passed over to be 13°. When this observation was recorded, the pendulum was supposed to have commenced its vibrations on a north and south line; but that was an error; it was really swung from east to west.

In repeating this experiment, I have been more anxious to vary the conditions, in a few arranged observations, than to accumulate many similar results. We have observed in four azimuthal planes; one of our balls weighed eight pounds, the other twenty pounds: one was an oblate, the other a prolate spheroid; suspension was effected at first by thoroughly softened catgut, afterwards by untwisted silk: we have compared small and large arcs, counted the periodical times of vibration in three planes, noted the direction of motion in the elliptic path of the pendulum, and estimated the length of its minor axis. We have recorded results when no ellipticity was remarked, and others in which its injurious effect was manifest.

The pendulum performs one complete vibration in 8″: from which its length is deduced =52·+ feet. The chord of the arc of vibration was usually taken at 14 feet, but was on some trials reduced to 7 feet. The graduated circle was 12 feet in diameter. Great care was used in starting the ball, which did not rotate, but presented the same face to the same quarter of the room, in whatever direction it was swung. The apartment was subject to air currents; the floor from which the suspension was effected though strong was large; and there was no method of securing exact verticality in the iron tube which carried the flexible catgut or silk.

From one or all of these causes it happened that ellipticity in the path was noticed in almost every experiment, and it might exist in all, and be unobserved if the minor axis did not exceed one-fourth of an inch. After abandoning several trials in which the minor axis was observed to increase rapidly, it was thought desirable to determine by experiment the effect of this elliptical swing on the angular movement of the pendulum plane (see exp. 5).

First Set. Deviation observed after given intervals of Time. Ball a prolate spheroid, weighing eight pounds. Suspension softened catgut.

Direction at origin.	Minutes of time.	Deviation.		Chord at origin.	Ellipticity.	
		Left.	Right.		Direction.	Minor axis.
			°	ft.		
1. E. and W....	60	......	13	14	None ob	served.
2. N. and S. ...	45	......	$11\frac{1}{2}$	14	None ob	served.

Supposing no ellipticity to have *existed* in these experiments, the results are—

Deviation in one hour from E. and W. line to right 13°·0
„ „ N. and S. „ 15°·3

Second Set. Time observed when deviation amounted to given angles. Ball an oblate spheroid, weighing 20 pounds. Suspension catgut.

Direction at origin.	Minutes of time.	Deviation.		Chord at origin.	Ellipticity.	
		Left.	Right.		Direction.	Minor axis.
			°	ft.		
3. E. and W.	*a.* 15·38	......	3	14	None ob	served.
	b. 23·40	......	5	...	None ob	served.
4. N. and S.	*a.* 15·0	......	3	14	Very m	inute.
	b. 24·44	......	5	...	Left.	$\frac{1}{2}$ inch.

Neglecting the ellipticity in these experiments, the results are—
Deviation in one hour from

E. and W. line...... *a.* 11·5, *b.* 12·9 } mean 12°·2+.
N. and S. line ...... *a.* 12·0, *b.* 12·1 } mean 12°·0+.

For obvious reasons—the force maintaining the pendulum plane being greatest in the beginning of the sweep (when the versed sine of the arc is greatest), and the action of the elliptical swing then least—the deviations in the first portions of the hour appear likely to be more correct than the average of the whole hour.

The suspension was now changed from softened catgut to untwisted silk. From some disarrangement connected with this change it happened that ellipticities were generated in every experiment, and nearly all were abandoned as useless on account of the great dimensions of the ellipse, which, growing as the arc of vibration lessened, sometimes acquired a minor axis of above three inches. Whatever the direction of the movement in the ellipse, its effect was to rotate the pendulum plane in the same direction; thus augment-

ing the deviation when the motion in the ellipse was to the right, and diminishing it when it was to the left. In most azimuths the elliptical motion was to the *left*. In the following example its effect was followed for an hour to the *right*.

Direction at origin.	Time.	Deviation.		Chord.	Ellipse.		Time to 1° from origin.	Successive times of 3°.	Accelerating effect of ellipse.
		Left.	Right.		Direction.	Minor axis.			
	m s		°			inches.	m. s.	m. s.	m. s.
5. E. and W.	0	......	......	14	none.	none.	......	14 6	
	7 3	......	$1\frac{1}{2}$	Not observed.	small.	small.	4 42	11 23	2 43
	11 23	......	3		......	$1\frac{1}{4}$	3 47		
	14 28	......	$4\frac{1}{2}$		......	......	3 13	8 35	5 31
	16 56	......	5		......	2	3 39		
	19 58	......	6		......	$2\frac{1}{2}$	3 19		
	24 16	......	8		......	3	3 2	7 40	6 26
	27 38	......	9		......	3+	3 4		
	34 18	......	12	7	......	$3\frac{1}{4}$	2 51	6 50	7 16
	42 8	......	15	Not obs.	......	$3\frac{1}{4}$−	2 48	7 50	6 16
	50 38	......	18		......	$3\frac{1}{8}$	2 48	8 50	5 36
	60 0	......	$26\frac{1}{2}$*		......	Not obs.			

When this experiment was recorded I had but slight expectation of being able to apply a correction to results which were so largely influenced by elliptical motion. While making it my attention was mainly directed to the rather difficult task of correctly estimating the minor axis of the ellipse (the most important of the elements for determining its rotatory effect), and I only twice recorded the length of the major axis, viz. at its origin, 14 feet, and, after the expiration of rather more than half an hour, 7 feet.

The Astronomer Royal, to whom the experiment in the state here set down was communicated, having kindly furnished me with an appropriate formula, I have resumed the consideration of what had appeared to me an unmanageable result. In this formula $\left(\frac{8}{3}\times\frac{a^2}{bc}=n\right)$ a is the length of the pendulum, b and c the semiaxes of the ellipse, n the number of complete double vibrations of the pendulum during the period of one rotation due to the ellipse. In this case eight such vibrations being performed in one minute, $\frac{a^2}{3bc}$=minutes of time to one rotation of the ellipse.

The first 12° of deviation were performed in $34^{m}\ 18^{s}$
or at the rate of 360° . . . . in 1029
or 17·15 hours,

the ellipse having its major axis varying from 14 to 7 feet, and its minor axis from 0 to $3\frac{1}{4}$ inches. Taking b and c at arithmetical means of their extreme values (in the case of the major axis this mean is something too great, and in the case of the minor axis something too small), we have

* Doubtful.

$$\frac{a^2}{3bc} = \frac{52^2}{3 \times 5{\cdot}25 \times {\cdot}0674} = 2547^{m},$$

and

$$\frac{1}{1029} - \frac{1}{2547} = \frac{1}{1726}\text{; whence}$$

360° are performed by the pendulum without ellipticity in 28·75 hours, and 12°·5 in . . . . 1 hour.

We have thus from the Minster experiments,—

a. Uncorrected for Ellipse.

Exp. 2. 4. On N. and S. line $\frac{15{\cdot}3+12{\cdot}0}{2} = 13^{\circ}{\cdot}65$ in one hour.

1. 3. On E. and W. line $\frac{13{\cdot}0+12{\cdot}2}{2} = 12^{\circ}{\cdot}60$.

β. Corrected for Ellipse.

Exp. 5. On E. and W. line . . . . . . 12·50.

Experiments in my House.

After many failures the apparatus became tolerably efficient, with a pendulum of 68·7 inches, as deduced from the vibrations, 22·64 in a minute. The balls used were a prolate spheroid weighing $6\frac{1}{4}$ lbs., and a globe weighing only $1\frac{1}{12}$lb. I have obtained the best results with the smaller weight. The suspensions have been silk, gutta percha, and various contrivances of points and sockets of agate, brass and steel. The best results have been obtained with gutta percha, and sockets of agate and steel. The arc mostly used was from 16 to 20 inches. The experiments were seldom continued beyond half an hour. By that time the chord of vibration was reduced to about 7 or 8 inches, and the errors of experiment were thought likely to be too great, with so short a radius, if longer continued. The ball, in whatever direction swung, presents the same face to the same side of the room.

Direction at origin.	Time.	Deviation.		Chord.	Ellipse.	
		Left.	Right.		Direction.	Minor axis.
	m		°	in.		
(6). N.E. and S.W.	0	......	0	18	No	ne.
	15	......	$3\frac{1}{2}$	....	None ob	served.
	30	......	7	....	None ob	served.
(7). N.W. and S.E.	0	......	....	18	No	ne.
	45	......	8	....	None ob	served.
(8.). N.E. and S.W.	0	......	....	18	No	ne.
	15	......	3	....	None ob	served.
	30	......	6	4	Left.	$\frac{1}{16}$ inch.
(9). N.W. and S.E.	9	......	....	18	No	ne.
	15	......	3	....	None ob	served.
	30	......	5·55	....	Left.	Minute.

From these experiments uncorrected for ellipse, we have,—

Exp. 6. 8. On N.E. and S.W. line $\frac{14+12}{2}=13^{\circ}\cdot0$ in one hour.

7. 9. On N.W. and S.E. line $\frac{10\cdot66+11\cdot10}{2}=10^{\circ}\cdot88$.

I have since made a great variety of experiments with this apparatus, which, notwithstanding the theoretical and practical disadvantage of working with so short a pendulum, I hope to render accurately effective, so that the angular deviation of the pendulum-plane may become an ordinary and easy experiment. It should, however, be tried in a glass case, and probably *in vacuo*.

4. "Note on instantaneous Photographic Images." By H. F. Talbot, Esq., F.R.S. &c.

"Having recently met with a photographic process of great sensibility, I was desirous of trying whether it were possible to obtain a truly instantaneous representation of an object in motion. The experiment was conducted in the following manner. A printed paper was fixed upon a circular disc, which was then made to revolve on its axis as rapidly as possible. When it had attained its greatest velocity, an electric battery, kindly placed at my disposal by Mr. Faraday, was discharged in front of the disc, lighting it up with a momentary flash. A camera containing a very sensitive plate of glass had been placed in a suitable position, and on opening this after the discharge, an image was found of a portion of the words printed on the paper. They were perfectly well-defined and wholly unaffected by the motion of the disc."

"As I am not aware that this experiment has ever succeeded, or indeed been tried, previously, I have thought it incumbent on me to lay an early account of it before the Royal Society."

5. "On the Impregnation of the Ovum, in the Amphibia (Second Series), and on the Nature of the Impregnating Influence." By George Newport, Esq., F.R.S., F.L.S. &c. Received June 19, 1851.

The author commences his paper by stating that, having given direct proof, in his former paper, that the spermatozoon is the impregnating agent, and also that the *liquor seminis* does not effect impregnation, he now proposes to detail some new experiments which bear on the views he then advanced; and especially with respect to the nature of the impregnating influence.

He first details some additional experiments with *solution of carmine*, with the object to show, that the result of one experiment mentioned in his former paper, in which he detected a small granule of carmine within the vitellary membrane, was attributable to the cause he then assigned—accidental injury to the egg; and he states that the results of his present investigations confirm him in the view then held,—that no natural perforation or fissure exists in the envelopes of the egg, either of the Frog or of the Newt, before, or at the

time of impregnation; and that the spermatozoon does not penetrate into, but only lies in contact with the envelopes.

He next gives the results of some experiments with solution of potass, in confirmation of his former observations; and further shows, the effect produced on the egg by immersion in solutions of potass and soda, with different proportions of the salts; and afterwards details the results of other experiments made to test some of the more remarkable ones by Spallanzani with regard to the effect of very minute quantities of the impregnating fluid. In these trials the author has proceeded by the mode of direct application of the fluid, and not by immersion of the eggs in large quantities of water, with small proportions of seminal fluid, the mode followed by Spallanzani. The result of the direct application through contact, *once only* with each egg, with the point of a pin wetted with the fluid, was, that this was sometimes sufficient to effect the commencement of segmentation, and consequent *partial impregnation*; while, if the fluid was allowed to drain off the pin, by continuing the contact for a few seconds, then complete segmentation, and full impregnation followed, and, other circumstances being favourable, an embryo was produced; and when the head of a pin was employed to apply the fluid, then the usual result was full impregnation; so that these results confirm those by Spallanzani. The author further states that it appeared to be of no consequence as to which surface of the egg was touched, the dark surface, light surface, or the side,—the result was the same.

He next proceeds to show, that when the egg is immersed in *pure* seminal fluid a *directly opposite result ensues.* Segmentation then seldom occurs, and the embryo is but rarely produced; and further, that the effect then produced on the egg is very similar in appearance to that of the *chemical action* of solution of caustic potass; the yelk becomes shrivelled and decays. These results he thinks are not explained by the views at present entertained respecting the nature of impregnation. The author then refers to the observations made by himself, and also by M. Quatrefages, which tend to show that no impregnation is effected, even by the contact of the spermatozoon with the egg, when all motion in the spermatozoon has entirely ceased; and he conceives that this fact, when considered with the results now obtained, leads to a new view of the subject.

Reference is then made to the views of Faraday, Mr. Grove, Matteucci, and others, respecting the correlations of the physical forces; and the author mentions that the relations of the vital to the physical forces were referred to by himself in 1845, and also in the same year by M. Mayer, and that his object now is to apply these views to the investigation of the function of impregnation. He thinks that impregnation is commenced, if not entirely completed, by what may possibly prove to be a new condition of force, *in*, and *peculiar to*, the impregnating agent, the spermatozoon, which he designates *sperm-force*, and distinguishes from the force of growth and development in cells, through which the spermatozoon is produced. He further distinguishes it from the force of *muscular contractility*, and

from that of the *nervous system*, and states that he regards the whole only as modifications of one common force, and as having correlations with the physical forces. In support of this view the author enters into details, and refers to some late analyses by Dr. Frerichs, to show that the spermatozoa, like muscle and nerve, not only have a definite structure, but also a definite chemical composition, and that this composition appears to be the same in different classes of animals. He thinks that the spermatozoa may thence be regarded as organs of a special modification of force, and that motion is the visible exponent of this form of force, since the spermatozoa are quite inefficient to impregnate when their motion has entirely ceased. The author further thinks that it is only by the adoption of views of this kind that the apparently contradictory results obtained are likely to be explained.

In the course of his observations the author states a remarkable fact, which he has repeatedly verified, and which he thinks is of importance, namely, that the *first cleavage or division of the yelk, in the egg of the Frog and Toad, corresponds in its line of direction to the longitudinal axis of the body of the embryo* of those animals; and this he proposes to show more particularly hereafter.

6. "The Human Iris; its Structure and Physiology." By Bernard E. Brodhurst, M.R.C.S. Communicated by Thomas Bell, Esq., Sec.R.S. &c. Received May 22, 1851.

The author commences by stating that the iris is an active fibro-cellular tissue, or that it may be considered to be a transition tissue from the ordinary fibro-cellular to the organic muscular: that it is a tissue differing from every other in the body; being possessed of a motor power exceeding that of any other tissue, yet differing in construction and appearance of fibre from those other tissues, the types of motion.

He remarks that the microscope shows that the fibres of the iris differ essentially from muscular fibre, whether striped, or of organic life: they are pale, easily separable and readily torn; but they resemble in no essential particular muscular fibre; indeed, the effect of galvanism on the iris is totally opposed to that produced on muscular fibre.

He observes that the nerves that pass to the iris are derived from both motor, sensitive and vegetative nerves; but voluntary motion is not supplied, neither sensation. The motions of the iris are wholly independent of the powers usually deemed motor; they are influenced primarily by the sympathetic system of nerves, through which motion is accorded without sensation, motion without design.

In death, the author observes, the iris assumes a median state, the pupil being neither dilated nor contracted. In health, it is contracted. During sleep it is contracted. During the presence of disease, the pupil is dilated, and so much dilated beyond its usual state, as the tonicity of the vegetative system is removed, as the presence of disease operates on the nutritive system to diminish not only the power of nutrition, but, in a like degree, tension of the visceral system; nutrition and tension

being as cause and effect of the healthy operation of this basic system of the animal œconomy. And as it is not essential to the motions of the iris, either to their performance or that they be understood, that they partake of any of those peculiarities, the distinguishing features of muscular tissue, and as we find that this membrane is obedient to those laws which are applicable to each organ under immediate sympathetic influence, and opposed to those phenomena which result from spinal and cerebral influence, it may be asserted that the contractility of the iris is, *primo loco*, the motor power of the sympathetic. For the iris is an irritable membrane with power alone of involuntary motion and tension, its active condition agreeing in these respects with vegetative life in general. And as animal death may be said to ensue when deep sleep takes possession of the senses, when those systems under spinal and cerebral influence are rendered inactive, to be fitted for renewed exertion on waking, it follows, that those organs which still remain active cannot be governed on the same principle, but must necessarily be subject to the sole remaining power, through which is accorded involuntary motion, motion which never tires, and tension its active condition.

The fimbriated edge of the ciliary body floats loosely in the posterior chamber around the lens, to produce, through the to and fro motion of each process (their aggregate number representing a circle), a current forwards or towards the iris. The force of this current is in a ratio to the pupillary opening, being increased as this is contracted, to produce, in proportion to its contraction, convexity of the iris. On the escape of the aqueous humour from the chambers, these processes fall down to form a serrated border upon the lens.

7. "On the Automatic Temperature-compensation of the Force Magnetometers." By C. Brooke, M.B., F.R.S. Received May 8, 1851.

After explaining the necessity of automatic temperature-compensation in these instruments in order to give the highest degree of accuracy to results deduced from the ordinates of the magnetic curves, the author infers from a reference to the formula expressing the conditions of equilibrium of the bifilar magnet, that the interval between the lower extremity of the suspension lines will be most advantageously submitted to some mechanical agency governed by change of temperature.

The object in view has been attained by attaching the lower ends of the suspension skein to the adjacent ends of two zinc tubes that are clamped to a glass rod which is attached at its middle point to the middle of the bar-magnet. When the temperature rises, the ends of the skein will evidently be approximated to each other by a quantity that is equal to the difference of expansion of the lengths of zinc and glass intervening between the clamps. The interval between the clamps is to be approximately determined by calculation, and corrected by experiment, so that the ratio of the expansion to the distance between the threads may be equal to the first term of the temperature coefficient.

In the balanced magnetometer the compensation is effected by

means of a small thermometer attached to the magnet, the stem of which is parallel to the axis of the bar. In this thermometer, the size of the bulb, its distance from the freezing-point and length of the scale, may be so proportioned to each other, that the second as well as the first term of the temperature coefficient will be represented in the correction.

8. "On the Reproduction of the *Ascaris Mystax*." By Henry Nelson, M.D. Communicated by Allen Thomson, M.D., F.R.S. Received May 22, 1851.

The author commences with a brief anatomical description of the *Ascaris Mystax*, found in the intestinal canal of the Domestic Cat; with more especial reference to the organs of generation in the two sexes. He traces the gradual formation of the semen; originally thrown off as seminal particles by the cæcal extremity of the tubular testicle, the exterior of each solid particle enlarges to constitute a cell, while the interior retains its consistency and forms a nucleus. The cell then acquires a granular protecting envelope, and in this state is introduced into the female. The solution of the protective envelope and the great enlargement of the seminal cell follow, and its nucleus is now seen to present a granular structure. The external granules of the nucleus coalesce to form a membrane, at first exactly resembling a watch-glass in shape, but by the contraction of its margin ultimately forming a curved cæcal tube. This is the true spermatic particle or spermatozoon, and is set free by the rupture of the seminal cell.

The generative apparatus of the female, commencing also in cæcal extremities, is next treated of, and the author draws particular attention to a transparent, narrow contractile portion, the oviduct, intervening between the ovary and uterus, as the part in which the ovule encounters the spermatic particles, and is by them fecundated. The cæcal end of the ovary likewise throws off a solid particle, which enlarging forms a germinal vesicle and spot. As the germinal vesicle travels slowly down the tubular ovary, it acquires a thick granular investment or yolk, secreted by the ovarian walls. The ovules now present a flattened triangular shape, are placed side by side, and form one solid mass. At the commencement of the oviduct however they become detached, separated from each other, and propelled singly along its interior. Here the gelatinous ovule meets the tubular spermatic particles, and is surrounded on all sides by them. They are at first seen to be merely applied against the ovule, but by degrees the margin of the latter presents a rupture, some of the vitelline granules are displaced, and the spermatic particles become imbedded in the substance of the yolk itself.

While the penetration of the spermatic particles is going on, a chorion, secreted by the oviduct, surrounds the ovule, forming a spherical envelope, within which the germinal vesicle, the granular yolk, and the imbedded spermatozoa, are enclosed. The spermatic particles after penetration are seen to swell, become transparent, and ultimately to dissolve. The vitelline granules likewise either disap-

pear altogether, or are transformed into others of a different colour; and, lastly, the germinal vesicle is destroyed.

By tracing the changes of the ovule in unfecundated females of the same species, the author finds the disappearance of the vitelline granules to be dependent upon, while the formation of the chorion is wholly independent of, the influence exerted by the spermatic particles on the ovule.

As soon as the vitelline granules and germinal vesicle have disappeared, the whole interior of the chorion is filled with a clear fluid, in which a few granules and the germinal spot are seen to remain. By swelling up this constitutes the embryonic vesicle and spot. A membrane separates from the interior of the chorion, and contracting on the granules forms a spherical yolk, in the centre of which is the embryonic vesicle. This is the perfect ovum. The subsequent divisions of the embryonic spot, vesicle and yolk are described; the author particularly pointing out the gyrations of the embryonic vesicle immediately after division. As soon as the whole interior of the egg has been filled by the subdivisions of the yolk, the external granules coalesce and form a continuous membrane internal to the chorion, which by gradual depression on one of its sides forms first a fleshy cup, and then, as the membrane of its concavity touches that of its convex surface, acquires the form of a ring. The ring divides at some point of its circumference, the extremities become pointed, and thus the young Ascaris receives its characteristic shape. The author has frequently repeated his observations with a view to their verification, and has employed the camera lucida to render the illustrative figures as accurate as possible.

9. "On Induced and other Magnetic Forces." By Sir W. Snow Harris, F.R.S. &c. Received April 29, 1851.

The question as to identity in the source of those several and mysterious powers of nature by which masses or particles are moved either toward, or from each other, being a question of deep physical interest, the author of this paper has been led to some further investigation of the nature and laws of magnetic force, in the course of which several new facts have presented themselves which he thinks not altogether unworthy of attention.

Magnetic attraction as commonly observed being found to depend on certain impressions made on the attracting bodies usually designated by the general term induction, it appears essential to the progress of any inquiry into the laws of those forces operating externally to a magnet through space, to commence with a rigid examination of the nature and mode of action of those inductive forces upon which the reciprocal force of attraction between the bodies immediately depends. These forces of induction may be considered as a series of successive or reverberating influences, operating between the near and opposed surfaces of the magnetic bodies. When, for example, a magnet is opposed to a mass of soft iron, a direct impression is first made on the iron by which the iron is rendered temporarily magnetic; this induced force operates in its turn by a

species of reverberation or reflexion upon the near pole of the magnet, and calls into play a portion of the magnetic force in the direction of the iron, which was previously operating toward the centre of the magnet; this action being once set up, may continue for a series of waves reverberating between the opposed surfaces, until the action sinks away as it were into rest. The author examines experimentally, by means of instruments, the principles of which he has already detailed in the Transactions of the Royal Society, this peculiar kind of action, and arrives at the following deductions relative to the laws of magnetic induction.

A limit exists in respect of induced magnetic force, different for different magnets, and varying with the magnetic conditions of the experiment, toward which the increments in the force continually approach, as if the opposed bodies were only susceptible of a given amount of induction under the existing circumstances.

Taking the force toward the limit of action, the amount of induction is in some inverse ratio greater than that of the simple distance; it was not however in any case found to exceed the inverse sesquiplicate ratio or $\frac{3}{2}$ power of the distance; as the distance is diminished the induction is as the distance inversely, but may in the mean time be as the $\frac{4}{3}$ or $\frac{5}{4}$ powers of the distances inversely, or near those powers. On further diminishing the distance, the induction was found in certain cases to be as the $\frac{2}{3}$ and $\frac{1}{2}$ powers of the distances, thus causing a series of changes in the law of magnetic attraction as commonly observed, which have hitherto greatly embarrassed the views of philosophers in their inquiries into this species of force. When the convergence is slow the induced force may not for a long series of terms appear to change, but when from any circumstance the convergence is accelerated, then the changes become more marked and successive. As a general result, however, the author is led to conclude, that magnetic induction is as the magnetic intensity directly, and from the $\frac{1}{2}$ to the $\frac{3}{2}$ power of the distance inversely.

In the course of these inquiries, it was found that the inductive action depended, not on the mass, but on the surface of the magnetic substance, and that magnetism, like electricity, exhibits a decrease of intensity when the surface of the iron upon which it is disposed is extended. A hollow cylinder of soft iron was carefully prepared in a lathe, and fitted with a solid interior core capable of being drawn out from within the cylinder; this compound body was exposed to the inductive action of a powerful magnetic bar, and the induced force estimated by the reciprocal force of attraction exerted between the mass and a cylinder of soft iron suspended from the author's magnetic balance, or from one arm of a light beam, set up in the way of a common balance. The degree of force being observed, the solid core was drawn out so as to extend the surface of the mass under induction. The intensity immediately declined, and again increased on replacing the solid within the hollow cylinder, being a result of exactly the same character as that produced by the extension of an electrified surface. When the interior solid

core was removed altogether, then the induced force remained unchanged, it being precisely the same whether the body were taken hollow or solid. In accordance with this result, hollow cylindrical magnets were found as susceptible of magnetic power as solid masses of the same temperament and dimensions; an unmagnetized solid and tempered steel cylinder, placed within a hollow tempered steel cylinder, does not become magnetic on touching the external cylinder in the usual way. The magnetism, however, of a hollow cylindrical magnet is partially destroyed by placing within it a cylinder of soft iron, or the reverse poles of another magnet; nor can a hollow cylinder of tempered steel having a solid core of soft iron be rendered magnetic by the usual methods of touch. These results, it is considered, supply the experiments thought by Mr. Barlow so desirable to confirm his deductions relative to the action of iron shells and balls on the compass needle, which he found to be as the $\frac{3}{2}$ power of the surface, whatever the weight and thickness of the iron.

The author now proceeds to notice the investigations of Hawksbee, Brook Taylor, Muschenbroek and others, and thinks the inquiries of these philosophers have not been sufficiently considered or appreciated; that instead of the results exhibiting anomalies and discrepancies, they are really necessary consequences of the more elementary laws of induction, and perfectly explicable upon the fundamental principles of magnetism. He endeavours to show, that by the changes in the law of the induction, as already stated, laws of force will arise perfectly coincident with the results arrived at by Hawksbee, Brook Taylor and others; that is to say, the law of force may appear to be as the $\frac{5}{2}$ power of the distance inversely, as found by Brook Taylor; or as the $\frac{3}{2}$ power inversely, as found by Martin; or in the inverse duplicate ratio of the distance, as observed by Lambert; or as the simple distance inversely, as determined by Muschenbroek in several cases; or it may be as the cubes of the distances inversely, as stated by Newton. Examples are given in which these several laws were found to obtain.

In examining the laws of magnetic repulsion, similar results are arrived at. The inductive forces here, however, are subversive of the existing polar arrangements; hence the apparent repulsion: so long as the existing magnetic polarities remain unchanged, the law of force will be generally as the second power of the distance inversely; when the distances are small, it will be inversely as the simple distance; when the inductive actions subvert the existing polarities, then the law of force appears irregular and subject to no regular variation, as observed in all the early experiments with repellent poles.

The author is led to conclude, that the apparent law of attractive force will be found to depend in certain cases on the distances at which the force operates, as referred to the total distance or limit of action. Taken between $\frac{2}{5}$ths and $\frac{5}{6}$ths of the limit of action, the force may be inversely as the third powers or cubes of the distances; taken between $\frac{2}{5}$ths and $\frac{3}{5}$ths of the limit of action, it may be in the

inverse sesquiduplicate ratio, or $\frac{5}{2}$ power of the distances; between $\frac{1}{3}$rd and $\frac{3}{5}$ths as the squares of the distances inversely. From the $\frac{1}{5}$th to $\frac{1}{2}$ of the limit of action it may be as the $\frac{3}{2}$ power of the distance inversely; within less than $\frac{1}{5}$th, it will be generally as the simple distance inversely.

On a further review of these laws of magnetism, it is evident that the immediate effect of an increase or decrease of distance, is an increase or decrease of the effective magnetism on which the total or reciprocal force depends. Thus taking the cases just quoted, it will be seen that the total force is always as the square of the induction, whatever be the resulting law of the attraction. Hence the force may as well be taken as the square of the quantity of effective magnetism directly, as some power of the distance inversely.

The author admits the difficulty in the way of the employment of such terms as quantity of magnetism, magnetic charge, and the like, and therefore only employs them according to the common acceptation of such terms, and not as referring to any particular hypothesis: he thinks there must necessarily be in such inquiries an element fairly enough expressed by the general term quantity as expressive of the relative or absolute magnitude of the cause, whatever it be, upon which the observed effects depend, and thinks it so far essential to obtain exact quantitative measures. In electricity we may estimate the charge conveyed into a battery by means of the unit measure, and we can at pleasure operate with one-half, one-third, &c. the quantity of electricity numerically expressed; but we have as yet no such measure in magnetism, and we are quite uncertain as to the quantity of effective magnetism in operation. The author hence endeavours to verify the law of magnetic charge just mentioned by a direct quantitative experimental process. A cylindrical rod of soft iron being surrounded by three successive coils of covered copper wire, was placed under the trial cylinder of the magnetometer and exposed to the operation of one or more precisely equal and similar batteries; one coil being appropriated to each battery. It is inferred that if one battery and one coil produced one measure of magnetism, two batteries and two coils would develope two measures, and so on; so that we should have only to determine the attractive force under this condition; now the attractive forces were found to be as the square of the number of batteries in action upon this cylinder, that is to say, as the square of the magnetism induced in the iron; hence the quantity of magnetism is as the square roots of the reciprocal forces. If therefore the reciprocal force between a magnet A and a cylinder of soft iron taken at a constant distance were represented by an equivalent of 4 grains, whilst the similar force with a magnet B at the same distance were represented by 9 grains, then the effective quantities of magnetism and operation in each case would be as $\sqrt{4} : \sqrt{9}$, that is as $2 : 3$.

Availing himself of this law, the author endeavours to deduce experimentally the magnetic development in different points of a regularly tempered and magnetized bar, taken between the magnetic centre and extremities; and he finds by a very careful manipulation,

that the magnetism in these points is directly as the distance from the magnetic centre; the reciprocal force on a small trial cylinder being as the squares of the distances from the centre.

Some striking analogies in the state of a magnetized steel bar and the common Leyden jar are noticed in this communication, from which it would appear that the conditions of electrical and magnetic force are precisely the same, and from which the author concludes that magnetic attraction is reducible, as in electricity, to an action between opposed surfaces; he thinks that a predisposition to identify these forces with that of gravity and other central forces has led many profound mathematicians and philosophers to question unduly the accuracy of every result not in accordance with such a deduction. He observes that Sir Isaac Newton considered "that the virtue of the magnet is contracted to the interposition of an iron plate, and is almost terminated by it, for bodies further off are not attracted by the magnet so much as by the iron plate*;" as also that this power is essentially different from gravity, "and in receding from the magnet decreases not in the duplicate, but almost in the triplicate proportion of the distance*," a result which has been shown to be perfectly consistent with experiments. Newton however has been supposed to have had "very inaccurate ideas of magnetic phenomena†;" it would be very difficult however to show from the little which this great author has advanced upon this subject in his grand work, the Principia, in what his views of magnetic action were defective; they appear on the contrary to be in most perfect accordance with experimental facts. In associating magnetic action with a law of the "centrifugal forces of particles terminating in particles next them," Newton never pretended to offer any theory of magnetism, but says with his usual diffidence, "whether elastic fluids do really consist of particles so repelling each other is a physical question," and "which he leaves philosophers to determine." On the other hand, a large amount of experimental research by Hawksbee, Brook Taylor, Whiston, Muschenbroek, and other eminent men, has been supposed by Dr. Robison as unworthy of confidence, and ill adapted to the object for which it was designed‡. The same learned writer thinks that magnetic attractions and repulsions are not the "proper phenomena for declaring the precise law of variation." Yet was it by these same attractions and repulsions that Lambert, and more especially Coulomb, deduced what this accomplished author considers as being the true law of force. The author of this communication is led to believe, that all the results of these inquiries, including the deduction of Newton, are not only consistent with, but necessary consequences of, the laws of induced magnetic forces, as he has endeavoured to prove, and that the action of magnetism as commonly observed is something different from what has been usually imagined. That future inquiries may lead to the identity in origin of magnetic and gravitating force he thinks not improbable; there may be some diffuse emanation through space, the

* Principia, Books 2 and 3. † Edinb. Ency. vol. xiii. p. 270.

‡ Mechanical Philosophy, vol. iv. p. 217.

source of gravity, and other central forces; and it is not impossible but that the relations of this medium to the particles of common matter may admit of considerable modification or change, and which may be the source of that peculiar power we find displayed in those bodies we consider as being magnetic and call magnets. It has been occasionally supposed that in the reciprocal force between magnets and iron there is a peculiar agency in operation, the law of which is disturbed by the new forces of induction liable to ensue in changing the distances. The author however is of opinion that such a notion is inconsistent with the course of nature; it is induction which constitutes magnetic action, there is no other form of action; when induction is not present there is in fact no action; we must hence look to these very changes for an explanation of variable magnetic force.

10. "Researches into the Identity of the Existences or Forces, Light, Heat, Electricity and Magnetism." By John Goodman, M.D. Communicated by Thomas Bell, Esq., Sec. R.S. &c. Received March 7, 1851.

In this communication the author describes the effects that were produced on a moderately sensitive galvanometer by exposure to the sun's rays, and which were observed by him during a period of four months, commencing on the 14th of November, 1850. The instrument is described as consisting of forty-six turns of covered copper wire, $\frac{1}{25}$th of an inch in diameter. The helix is blackened with ink at its southern extremity, and has a single magnetized sewing-needle suspended by about sixteen inches of silken fibre in its centre. The dial, which is of card-board, and divided into the usual number of degrees, rests upon the upper surface of the helix, and shades it from the ordinary light or sun's rays, except at its extremities, and occasionally some portions of the lower bundle of wires; and when the sun is very low the rays may be seen also to illumine to some extent the surface of the upper bundle. The indicator is formed of a slender filament of light wood in the usual manner, and the whole is enclosed in a glass shade. This instrument was placed for experiment in a window having a southern aspect; and whilst the sun was strongly shining upon it, it was frequently observed that there could not be obtained, either on account of vibrations or the erroneous condition of the instrument, any true indications. On shading the instrument from the sun's rays by a screen, the vibrations ceased, and the needle again adjusted itself north and south.

On removing the screen the needle began again to vibrate, and was soon discovered to become stationary at some distance from zero, indicating the transmission of a current in the helix. This deflection of the needle was soon found to be always, under the same circumstances, in the same direction, and to give indications of a current corresponding to the brightness of the sun.

This action appeared to depend upon the incidence of the sun's rays upon the south extremity, and some of the lower or upper bundle of wires only of the helix; for when they began to illumine

the opposite extremity, either very slight indications, or a neutral result, constant vibrations, or the movement of the needle some degrees in the opposite direction, were always observed. The maximum deflection, at any time attainable by the galvanometer, when the sun was quite unclouded, was about 12°, generally only 10°. It may be observed that in all these experiments the power of the rays was probably somewhat diminished, by passing though the glass pane of the window, and through the glass shade of the instrument itself.

In order to show that the effect was not thermo-electric action, the extremities of the helix were removed from their mercury cups and wrapped in paper, so as to exclude the mercurialized portion of the copper from the action of the sun's rays; but no alteration occurred in the ordinary results of the experiments. There is, moreover, the author considers, no evidence on record of any thermo-electric action ensuing from the application of heat to copper wire alone, nor without the formation of a complete electrical circuit. But in these experiments hitherto the completion of the circuit had not been attempted. During the course of the experiments the circuit was established by means of a connecting wire between the mercury cups, and the circuit was again and again completed, and as frequently broken, without any deviation occurring in any of the results, either during the progression, stationary condition, or decline of the needle.

That these phenomena were the result of the action of the sun's rays upon the helix itself, was further shown, from the circumstance that when the sun remained clouded for days together, there was no deflection of the needle; that when the helix was partly shaded by a pillar, or the window-frame, the instrument indicated an amount of current corresponding to the number of coils of wire illumined; and *that the illumination of the whole bundle of wires* at the southern extremity of the helix was necessary to produce the usual results, for when a burning lens of high power was employed to condense the rays and throw them in a focus upon one or two wires only, no deflection of the needle was observed. It was also further shown that the action of the rays upon the helix was attributable to that portion situate chiefly at the southern extremity, for the whole instrument was in a variety of ways and at different periods shaded from the solar rays; but its results were unaffected, unless the south end was obscured, when the needle immediately declined; or the north end was illumined when the deflections were lessened, or the motion of the needle took place in the opposite direction.

A pile of red-hot burning embers held in the vicinity of one extremity of the helix caused a slight deflection of $\frac{1}{2}$°, and when held at the opposite extremity, caused a deflection in the opposite direction.

The author states a remarkable circumstance, viz. that vibrations and neutral action were observed during bright sunshine about the 11th of December, and again on the 23rd of January; that previous to the former period the deflections of the needle were to the *left-hand*; between these two periods they were to the *right-hand*; and

after the latter period always to the left, after a given hour of day During the early sun, however, they were to the *right-hand*, and as the sun approached a given altitude, they were invariably to the *left-hand*. Deflections observed during the summer season were also to the *left-hand*; but those of the early sun were not submitted to the test.

On testing the instrument with a voltaic pair, it was shown that the current passed from south to north *above the needle* with the *early sun*, or when the indicator deflected to the right-hand, and *beneath the needle* with the rays which proceeded from a *considerable elevation*, or when the needle deflected to the left-hand.

In conclusion, the author states that the results of these experiments evince to his mind more than ever the *unity of force*; and that experimental evidence appears to justify the conclusion at which he has long since arrived, *that there is one, only, universal force in nature, which is modified by the accidental and varied conditions to which it is subjected, but that its essential nature and characteristics are at all times unchangeably the same.*

II. "On the Mean Temperature of the Observatory at Highfield House, near Nottingham, from the year 1810 to 1850." By Edward Joseph Lowe, Esq., F.R.A.S. Communicated by Marshall Hall, M.D., F.R.S. Received May 3, 1851.

The object of the author in this communication is to connect the series of thermometrical observations made by the late Matthew Needham, Esq., at Lenton House, at the distance of only 200 yards from the observatory of Highfield House, with those made by himself from 1842 to the present time at the latter place. He procured Mr. Needham's observations from the Committee of the Bromley House Library, Nottingham, and also the instrument with which they were made, and which, upon comparison with his own standard, was found by Mr. Glaisher to be correct.

Mr. Needham's observations were registered at 8 A.M. and 11 P.M., and to the monthly means of these records corrections have been applied to convert them into mean monthly values. Those made by the author were registered at 9 A.M. and 9 P.M., and these, together with the highest and lowest readings of self-registering thermometers, have been subjected to the same process.

The following tables deduced from the observations are given in the paper:—

1. The mean temperature of each month at Highfield House from 1810 to 1850.

From this table are deduced the mean temperature of each month from all the observations, viz.

January 36°·2; February 38°·9; March 42°·4; April 47°·6; May 53°·6; June 58°·7; July 61°·1; August 60°·2; September 56°·6; October 50°·0; November 42°·9; December 39°·1.

2. The highest and lowest monthly mean temperature in every year, from 1810 to 1850, with the amount of difference of temperature.

From this table it appears that the coldest month in the year has occurred in January 22 times; in February 10 times; in March once; and in December 8 times.

The hottest month in the year has occurred in June 5 times; in July 26 times; in August 12 times; and in September once.

The coldest month in the whole period occurred in January 1814, the mean temperature being 26°·8.

The hottest month during the whole period occurred in July 1847, the mean temperature being 68°·8.

The means of all the differences between the hottest and coldest month in every year is 27°·2: the least difference occurred in 1828, viz. 21°·3; the greatest difference in 1814, viz. 35°·0.

3. The excess of the monthly mean temperature in every year, above the temperature of the month from all the years.

The means of the numbers in each column of this table, taken without regard to sign, gives the variability of the temperature in spring 2°·1; in summer 1°·7; in autumn 2°·0; in winter 3°·0.

The greatest difference in the monthly means in spring is 11°·9; in summer 12°·5; in autumn 13°·9; in winter 18°·3.

The coldest year in this series was 1814, when the mean annual temperature was only 45°·0: the hottest year was 1846, the mean annual temperature being 51°·4.

4. The mean temperature in every month in successive groups of 10 years, and for the whole year.

5. The mean temperature in quarterly periods in successive groups of 10 years.

From this it is stated that the mean temperature of the 1st quarter is 39°·5; of the 2nd, 53°·3; of the 3rd, 59°·3; of the 4th, 44°·0.

6. The mean temperature in spring, summer, autumn and winter, in successive groups of 10 years.

From this it is concluded that the mean temperature of spring is 47°·8; of summer 60°·0; of autumn 49°·9; of winter 38°1.

Cold springs occurred in 1810, 12, 14, 16, 17, 37, 38, 39, 42, 45, 49 and 50; and the mean of the temperatures of these springs is 45°·5. The coldest spring was that of 1837, the mean temperature being only 43°·3.

Hot springs occurred in 1811, 15, 19, 22, 23, 27, 28, 30, 31 and 41; and the mean of the temperatures of these springs is 50°·4: the hottest spring was that of 1841, the mean temperature being as high as 51°·4.

Cold summers occurred in 1816, 17, 21, 23, 41, 43 and 49; and the mean of the temperatures of these summers is 58°·0. The coldest summer was that of 1816, the mean temperature being only 57°·3.

Hot summers occurred in the years 1818, 24, 26, 31, 46 and 47; and the mean of the temperatures of these summers is 64°·0. The hottest summer was that of 1846, the mean temperature being as high as 65°·0.

Cold autumns occurred in the years 1814, 16, 20, 29, 36, 37, 38, 42, 44, 45, 49 and 50; and the mean of the temperatures of these

autumns is 47°·8. The coldest autumn was that of 1849, the mean temperature being only 47°·0.

Hot autumns occurred in the years 1810, 11, 18, 21, 27, 28, 40 and 46; and the mean of the temperatures of these autumns is 52°·3. The hottest autumn was that of 1818, the mean temperature being as high as 54°·5.

Cold winters occurred in 1814, 16, 20, 23, 30, 38, 41, 45 and 47; and the mean of the temperatures of these winters is 34°·4. The coldest winter was that of 1814, the mean temperature being only 32°·7.

Hot winters occurred in 1822, 24, 28, 34, 35, 46, 48 and 49; and the mean of the temperatures of these winters is 41°·5. The hottest winter was that of 1834, the mean temperature being 43°·3.

12. "On Depressions of the Wet-bulb Thermometer during the Hot Season at Ahmednuggur, in the Deccan." By Colonel Sykes, F.R.S. &c. Received June 17, 1851.

The author states that he is indebted to Major William Coghlan for the tables of hourly depressions of the wet-bulb thermometer during the months of March and April of the present year, which form the subject of this communication, and which are a necessary supplement to his paper recently published in the Philosophical Transactions. The observations at Ahmednuggur, lat. 19° 05′ 49″ N., long. 74° 48′ 10″, elevation above the sea 1911 feet, which were undertaken by Dr. Forbes Watson, commenced on the 18th of March, and were continued to the 14th of April inclusive. They were made hourly from 6 A.M. to 9 P.M., giving 16 hourly records daily; but on the 24th and 29th of March, and on the 4th, 8th and 10th of April, they were continued throughout the twenty-four hours. The instruments employed were a dry- and a wet-bulb thermometer, by Adie, perfectly alike and giving precisely the same indications when both were dry, and a self-registering thermometer. They were suspended on a platform attached to a window under the verandah of the house, with a N.W. exposure, and were protected from radiation and reflexion of heat from the ground. As, from some preliminary observations, it appeared that the depression of the wet-bulb varied in every case with the intensity and duration of the draught of air upon it, in each observation a slight current of air was produced by a fan near the mouth of a funnel, the small end of which abutted on the wet-bulb, and the operation was continued until no further depression of the thermometer could thus be produced; a stronger current of air was then forced on the bulb by means of a large double bellows; and the result of each operation was recorded.

To obviate the anomalies which might arise from single observations, and to fix a mean state, for each hour, of the temperature of the air, the temperature of evaporation, and the mean depression of the wet-bulb, the means of these elements have been taken and are presented in a table. In this table are also given the dew-points as determined by means of Mr. Glaisher's factors and by Dr. Apjohn's

formula, with the differences by the two methods. The author remarks that the first feature which presents itself, in running the eye over this table, is the enormous amount of the depression of the wet-bulb compared with our European experience. In March, the mean depression at no hour was less than 14°·8 at 7 A.M., increasing to 29°·6 at 3 P.M.; in April, the mean depression was never less than 17°·3 at 7 A.M., increasing to 29°·9 at 3 P.M.; and many observations necessarily much exceeded the maxima means. The next feature is the increase of the mean depression with that of the mean temperature, from 6 A.M. until 3 P.M., and then a decline with the decline of temperature until 9 P.M.; but not in the same ratio as the increase in the morning. With reference to the practical application of these observations with a view to determine the amount of moisture in the atmosphere, or to fix the dew-point, the author remarks that it will be seen from this table that Mr. Glaisher's factors give a higher dew-point than Dr. Apjohn's formula, varying in March from 6°·1 at 8 A.M. to 11°·9 at 6 P.M., and in April from 5°·6 at 7 A.M. to 10°·4 at 9 P.M.; and these varying discrepancies do not appear to have gradations of increment or decrement dependent upon increase or diminution of mean temperature, or increase or decrease of the depression of the wet-bulb. These remarks apply to the means of the observations; but with reference to isolated observations, the discrepancies by the two methods become much greater. On the 9th of April, at 8 P.M., the temperature of the air being 97°, the wet-bulb with a moderate draught 60°·5, and with a strong draught 60°, the depressions were respectively 36°·5 and 37°, and the dew-point for the latter depression determined by Mr. Glaisher's factors would be 41·5, and 12·6 by Dr. Apjohn's formula. In illustration of this part of the subject the author gives an extract of a letter from General Cullen, from which it appears that at Cochin on the Malabar coast, the temperature of the air being 96°, the wet-bulb 61°, the dew-point by Jones's hygrometer 38°, the dew-point by Mr. Glaisher's factors would be 43°·5, and by Dr. Apjohn's formula 22°·1.

13. "On a General Law of Density in saturated Vapours." By J. J. Waterston, Esq. Communicated by Lieut.Colonel Sabine, R.A., V.P. and Treas. R.S. &c. Received June 19, 1851.

The author of this paper commences by stating that the relation between the pressure and temperature of vapours in contact with their generating liquids has been expressed by a variety of empirical formulæ, which, although convenient for practical purposes, do not claim to represent any general law; and that some years ago, while examining a mathematical theory of gases, he endeavoured to find out, from the experiments of the French Academy, whether the density of steam in contact with water followed any distinct law with reference to the temperature measured from the zero of gaseous tension (situated at −461° Fahr. by Rudberg's experiments, confirmed by Magnus and Regnault). To avoid circumlocution, he calls temperatures measured from this zero G temperatures, and observes

that if t represents the G temperature, Δ the density of a gas or vapour, and p its elastic force, the equation

$$t\Delta=p$$

represents the well-known laws of Marriotte and of Dalton and Gay-Lussac. He then states that, as the function which expresses a general relation between p and t, in vapours, must include a more simple function expressing a general relation between Δ and t, the proper course seemed to be to tabulate the quotients $\frac{p}{t}$ from the experiments of the Academy, and to project them in a curve. For reasons connected with the *vis viva* theory of gases, which represents the G temperature as a square quantity, he projected these quotients or densities as ordinates, to the square root of the G temperatures as abscissæ; and found that the curve traced out was of the parabolic kind, but of a high order. Considering the density as a cubic quantity, the cube roots of the densities were set off as ordinates to the same abscissæ, and the author was gratified to find that the resulting curve was the Conic Parabola. To ascertain whether this was accurately the case, the square roots of these ordinates, corresponding to the sixth roots of the densities, were set off to the same abscissæ, that is the square roots of the G temperatures. The result is shown in a chart, in which, as the author observes, the points determined from the observations range with great precision in a straight line, any slight divergence being sometimes to the right and sometimes to the left; precisely as might be expected from small errors of observation. Other series of experiments on steam were projected in a similar manner, and it was found that, although no two exactly agreed with each other, each set ranged in a straight line nearly. The vapours of ether, alcohol and sulphuret of carbon, were found to conform to the same law, as were likewise M. Avogadro's observations on the vapour of mercury, and Faraday's experiments on liquified gases (Phil. Trans. 1845). Of these last olefiant gas is remarkably in accordance with the law, as are nitrous oxide, ammonia, cyanogen, sulphurous acid, and carbonic acid at the upper part of its range; but muriatic acid, sulphuretted and arseniuretted hydrogen, do not show the same regularity.

The co-ordinates of the points being the square root of the G temperatures and the sixth root of the densities, the equation to the straight line which passes through the points expresses the sixth root of the density in terms of the G temperature. The constants to be determined in this equation are the inclination of the straight line to the axis of x or that on which $\sqrt{t}$ is measured, and the distance from the origin at which it cuts this axis, calling the cotangent of this angle h, and this distance g, Δ_1, Δ_2 densities at G temperatures t_1, t_2,

$$h=\frac{\sqrt{t_2}-\sqrt{t_1}}{\sqrt[6]{\Delta_2}-\sqrt[6]{\Delta_1}} \text{ and } g=\sqrt{t_1}-h\sqrt[6]{\Delta_1}.$$

The constants g and h being thus determined from two observations, the equation for the density at any other G temperature is

$$\Delta=\left\{\frac{\sqrt{t}-g}{h}\right\}^6;$$

and for the pressure

$$p=\left\{\frac{\sqrt{t}-g}{h}\right\}^6 t.$$

The several equations, with the numerical values of the constants g and h, for the series of observations previously referred to and represented on the chart, are then given, the G temperatures being in degrees of Fahrenheit's scale, and the values of h being calculated so as to give the pressure in inches of mercury.

The author remarks that the observations on the vapour of water below 80° show a small excess of density above what is required by the line corresponding to those at higher temperatures; and that it is a curious circumstance that the law of expansibility of water also becomes disturbed at about the same temperature. In proof of this, the observations of M. Despretz (Ann. de Chim. vol. lxx.) being projected, by making the volume the ordinate to the square root of the G temperature as abscissa, these observations above 25° C. or 77° F. give in the most exact manner a conic parabola; but below 77° they no longer give that curve.

The equation to the parabola for temperatures above 77° F. is $a(v-\theta)=(\sqrt{t}-\varphi)^2$, in which v is the volume at the G temperature t, in terms of its volume unity at 39°·2 F. or 4° C. (its point of maximum density), $a=352{\cdot}38$, $\theta={\cdot}99872$, and $\varphi=21{\cdot}977$ or $\phi^2=483°$.

The law of the increase of density and temperature in saturated vapours having a certain analogy with the law of increase of density and temperature in air while suddenly compressed or dilated, the author next discusses the latter subject in a manner similar to that in which he had discussed the former. From this discussion he draws the following conclusions:—

1. When air is compressed or dilated, the G temperature varies as the cube root of the density; and the tension as the 4th power of the G temperature, or the cube root of the 4th power of the density.

2. The mechanical force exerted by a given quantity of air while expanding from one density to another, is proportional to the difference of the cube roots of these densities, or to the difference of their G temperatures: hence the fall of temperature is proportional to the force expended.

3. The mechanical force exerted upon a given quantity of air, while compressing it from one density to another, is proportional to the difference of the cube roots of these densities, or to the difference of their G temperatures: hence the rise of temperature is proportional to the force exerted.

4. The total mechanical force exerted by a volume of air of a given tension, while expanding indefinitely, is equal to that tension acting through three times the volume.

5. The total mechanical force exerted by a volume of air while expanding indefinitely is proportional to its G temperature.

6. A given quantity of air while expanding, under a constant pressure, from one temperature to another, exerts a mechanical force equivalent to one-third the difference of temperature; and the quantity of heat required to change the temperature of air under a constant pressure, is four-thirds of that required to effect the same change of temperature with a constant volume.

The author concludes by observing that it is singular that these simple and, he considers, important deductions from MM. Gay-Lussac and Welter's experiments, have been overlooked by the eminent mathematicians who have elaborately discussed this subject. The artificial position of the zero-point on the ordinary scales of temperature may perhaps account for this by its tendency to confine our ideas. Dalton's and Gay-Lussac's law of expansion seems imperatively to have required that, in all computations having reference to gases and vapours, the temperature should have been reckoned from the zero of gaseous tension; yet it has not been so; and it is impossible to avoid the conclusion, that if it had been otherwise, if no other temperature but what we have had so often to refer to as the G temperature had been indicated in their analyses, we should have profited more by their labours, and been further advanced in the science of heat and elastic fluids.

The Society then adjourned over the vacation to Thursday the 20th November, 1851.

November 20, 1851.

COLONEL SABINE, R.A., V.P. & Treas., in the Chair.

George T. Doo, Esq. and Dr. Hofmann were admitted into the Society.

No paper was read; the time of the Meeting being occupied by reading the Minutes of the last Meeting, and the titles of the numerous presents received during the recess.

November 27, 1851.

COLONEL SABINE, R.A., V.P. & Treas., in the Chair.

A paper was in part read, entitled "Experimental Researches in Electricity. Twenty-eighth Series. On Lines of Magnetic Force; their definite character; and their distribution within a Magnet and through Space." By Michael Faraday, Esq., D.C.L., F.R.S., &c.

December 1, 1851.

At the Anniversary Meeting,

The EARL OF ROSSE, President, in the Chair.

Dr. Wallich, on the part of the Auditors of the Treasurer's Accounts, reported that the total receipts, during the past year, including a balance of £156 18*s.* 8*d.*, amounted to £3938 9*s.* 6*d.*; and that the total expenditure, during the same period, amounted to £3791 1*s.* 0*d.*, leaving a balance in the hands of the Treasurer of £147 8*s.* 6*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The President announced that Sir Philip Egerton, Bart., had been nominated by the Council a Trustee of the Soane Museum.

List of Fellows of the Royal Society deceased since the last Anniversary (1851).

On the Home List.

His Majesty, Ernest Augustus, The King of Hanover.

John James Audubon, Esq.
John Baron, M.D.
Henry Beaufoy, Esq.
The Right Hon. Lord Bexley.
James Ebenezer Bicheno, Esq.
Michael Bland, Esq.
John Burns, M.D.
Maj.-Gen. Sir Stephen Chapman.
Admiral Sir Edward Codrington.
Thomas Stephens Davies, Esq.
General Lord de Blaquiere.
Thomas Galloway, Esq.
General Sir James Gordon, Bart.
John Greathed Harris, Esq.
John Kidd, M.D.
Charles König, Esq.
The Rt. Hon. Viscount Melville.
Major-Gen. Sir William Morison.
The Marquis of Northampton.
George Pemberton, Esq.
Richard Phillips, Esq.
Captain Daniel Ross.
Sir Francis Simpkinson.
Rev. John Pye Smith, D.D.
William West, Esq.
John Williams, Esq.

On the Foreign List.

C. C. J. Jacobi.	Jens Christian Oersted.
Henry Frederick Link.	Heinrich Christian Schumacher.

Defaulters.

E. H. Baily, Esq., R.A.	E. R. Daniell, Esq.

Withdrawn.

Sir Howard Elphinstone, Bart.	Thomas Jones.
Rev. F. W. Hope.	

Election Void.

John Russell Hind, Esq.

List of Fellows elected into the Royal Society since the last Anniversary (1850).

On the Home List.

His Grace the Duke of Argyll.	John Russell Hind, Esq.
Charles Cardale Babington, Esq.	Augustus William Hofmann, Esq.
Thomas Snow Beck, M.D.	Thomas Henry Huxley.
Charles James Fox Bunbury, Esq.	William Edmund Logan, Esq.
George T. Doo, Esq.	James Paget, Esq.
Edward B. Eastwick, Esq.	George Gabriel Stokes, Esq.
Captain Charles M. Elliot.	William Thomson, Esq.
Captain Robert FitzRoy, R.N.	Augustus V. Waller, M.D.

The President then addressed the Meeting as follows:—

Gentlemen,

The year that is passed, though not remarkable for any very startling discovery, has contributed a full average to the advance of human knowledge: in all directions there has been steady progress, new facts springing up in rapid succession, the fruits of inquiries carried on with perhaps more energy, with more system, and upon a greater scale than at any previous epoch in the history of science. Our continental neighbours, happily exempt from internal troubles, have been free to pursue their scientific labours; and our brethren in America, with that energy which is daily winning for them such signal triumphs in the arts of peace and civilization, though later in the field, yet fresh and full of hope, have taken a distinguished place.

I need hardly remind you that at home the indications of active progress are no less striking. Here we have had an unusual number of papers, and the size of the last volume of the Transactions is evidence that a large proportion were of sterling worth. Many also have expressed their readiness to undertake new researches on re-

ceiving a certain amount of assistance from the Government Grant; and although no one has proposed any very extensive series of experiments, still many facts no doubt will be obtained in various departments of science,—a valuable addition to human knowledge; but, scattered over science at large, they will not of course make that brilliant display which they would do if concentrated, as in M. Regnault's inquiry, on one specific object.

I may also add, that many distinguished men have recently come forward here as candidates, and I am happy to say that so far the new Statute appears to be working very well, and as yet I do not see any probability that men of unquestioned merit will have too long to wait for admission.

Your Council having awarded the Copley Medal to Professor Owen, it becomes my duty to give some account of his discoveries. They cannot be described by a mere reference to one or two great works: they have appeared successively in a variety of publications, and are so numerous, that to notice each, however slightly, will almost take up more time than I can venture to ask for. Any further general observations would therefore be inadmissible; and thanking you and the Council for the kind assistance I have received during the past year, and indeed on all occasions, I will at once proceed to state the grounds of the award of the Copley Medal.

The Contributions to Comparative Anatomy and Physiology, made by Professor Owen in published works, began to appear about the time of the decease of Cuvier in 1832, and have been continued with brief interruptions since that period to the present time: the mantle of that great man seems to have descended, at his death, upon the shoulders of our distinguished countryman; and it is not a little interesting to remark how this circumstance has produced, as it were, an uninterrupted succession of important discoveries in these and the collateral sciences, during a period, already passed, of sixty years, by two men of different countries indeed, but the character of whose minds, and the originality and importance of whose discoveries and generalizations, have placed them on an eminence, not reached by any other philosophers of modern times, in the same branches of knowledge.

Professor Owen's earlier works are principally devoted to making known the labours of John Hunter, and facilitating, by the construction of an elaborate catalogue, the study of his unrivalled collection of anatomical preparations in the Museum of the Royal College of Surgeons.

It is unnecessary for me to do more than allude to the masterly manner in which this arduous task was performed. In 1840, the ten years' labour was completed by the publication of the fifth and last volume of a catalogue, which, for laborious research, for new and important views in anatomical and physiological science, is unrivalled in any age or country; and the Council of the College, in the advertisement to the last volume of the Physiological Catalogue, express " their great gratification in acknowledging the unremitting labour bestowed on the work by Professor Owen, to whom its pub-

lication has been exclusively confided." From the date of the publication of this catalogue, including as it does not only the description of 3790 Hunterian dissections, but also seventy-eight engravings of minute and elaborate drawings, together with the general observations left by Hunter, the true position of this great man in natural science became manifest, and a more just and ample recognition of his merits was produced, particularly on the continent.

These however were not the only labours of Professor Owen in connexion with his official duties. In addition to some minor catalogues and other works, I must not omit to mention the two well-known monographs, the 'Memoir of the Pearly Nautilus,' published under the auspices of the College in 1832; and that on the skeleton of a gigantic extinct Sloth (*Mylodon robustus*), published ten years afterwards.

The merits of these publications are fully recognised, and have received the highest praise from continental anatomists; and it is remarkable that the doubts which were expressed and long entertained, as to the accuracy of Professor Owen's restoration of the animal of the Pearly Nautilus to its shell, were completely dissipated by the first complete example obtained by the eminent Professor of the Garden of Plants, M. Valenciennes.

Our time will only allow me to take a slight glance at the learned and elaborate modification of the classification of the *Cephalopoda*, the reference of the spirula and the curious fossil Belemnite to the higher or dibranchiate order of this class of animals. For his memoir on certain of this family, with their soft parts fossilized, which was published in the Philosophical Transactions for 1844, the Royal Medal was awarded. This class of animals has been still further elucidated in Professor Owen's papers in the Zoological Society's Transactions, and in an elaborate article in the first volume of the Cyclopædia of Anatomy and Physiology.

The first communication received by the Royal Society from Professor Owen was on the Mammary Glands of the *Ornithorhynchus paradoxus* in 1832. These organs had been originally described by Sir Everard Home as masses of fat; they were afterwards recognised by Meckel as mammary glands in 1824: but their true nature was again disputed by Geoffroy St. Hilaire, who considered them as scent glands.

Professor Owen determined the question by observing the phases of change relatively between the ovaries and the glands in question, and established their true mammary nature, by the dissection in 1831 of no less than five female Ornithorhynchi and one Echidna; a doctrine, which was afterwards confirmed by observations made in Australia on the secretion itself of the gland.

In 1834 there appeared in the Philosophical Transactions Professor Owen's paper, describing impregnated specimens of Ornithorhynchus. In this paper he shows, by the structure of the ovisac, of the corpus luteum, and of the uterine ovum, that the latter must be developed *in utero*, and the young be born alive. He infers, from the structure of the chorion, that no placenta will be developed. The

problem still remained, how a quadruped, with a beak like a duck, could suck, or in any way obtain milk from a mammary organ without a nipple. In 1834 Professor Owen received specimens of young, and apparently newly-born, *Ornithorhynchi*, from Mr. George Bennett and Dr. Weatherhead: their form and anatomy are minutely described in a paper in the Transactions of the Zoological Society. The oral orifice was there shown to be exactly adapted to be applied to the areola of the breast on which the lactiferous ducts terminate, and to receive the milk that is injected into the mouth by a muscle that surrounds the large mammary gland*. The remains of fœtal peculiarities in these young specimens confirmed the inference from the structure of the ovum that the Ornithorhynchus was viviparous, but implacental.

Professor Owen's next step was to settle the questions undecided on the generation of marsupial animals—the period of uterine gestation, the exact condition of the new-born young, the mode of its passage to the external pouch, and the term of its suspension to the pendulous nipple. On all these points science was, as yet, uninformed. The Kangaroo (*Macropus major*) had bred in captivity, in both France and England. Professor Owen took advantage of the opportunities which the menagerie of the Zoological Society afforded to obtain exact data on the chief points which most needed elucidation. You will find the account of his experiments in the paper 'On the Generation of the Marsupial Animals,' in the Transactions of the Royal Society for 1834. The period of uterine gestation of the Great Kangaroo is shown to be thirty-eight days; the new-born animal is but one inch in length, naked, blind, with hind-legs and tail shorter than the fore-legs. He ascertained that the mother transferred her minute and delicate progeny from the vulva, to the nipple concealed in the pouch, by means of her lips; that the embryo instinctively adheres to the nipple, and is suspended to it for a period of six months.

In the Philosophical Transactions for 1837 appeared a memoir from Professor Owen's pen, describing certain peculiarities in the brain of the Marsupialia, especially the absence of the corpus callosum. The same condition he subsequently discovered in the Ornithorhynchus and Echidna. This and other peculiarities of structure in the sanguiferous, osseous and dental systems, led Professor Owen to suggest a modification of the classification of the Mammalia, which Cuvier had adopted in his last edition of the 'Règne Animal.' Deeming modifications of brain of more importance than those of the ungual phalanges, and connecting the higher development of the commissural system of the brain with the longer sojourn of the fœtus in the womb and its more intimate union therewith, Professor Owen, in his paper 'On the Classification of the Marsupialia,' in the Transactions of the Zoological Society for 1839, groups together all the Mammalia which have a placenta under any form, and which have

* An analogous arrangement had been previously shown to exist in the Kangaroo, by the late Mr. John Morgan.

the corpus callosum, in a primary *subclass,* under the name of '*Placentalia*'; the rest form the *subclass* '*Implacentalia,*' and this includes the orders *Marsupialia* and *Monotrema.* For a further development of these views, and of the organization of the *Implacentalia,* I may refer to Professor Owen's admirable memoir 'On the Osteology of the Marsupialia' in the Transactions of the Zoological Society, and to the articles 'Monotremata' and 'Marsupialia' in the Cyclopædia of Anatomy and Physiology.

Professor Owen further displayed modifications of the Cuvierian system on anatomical grounds, in his paper on the *Dugong* in the Proceedings of the Zoological Society for 1838, in which he separated the *Herbivorous* from the true or *Carnivorous Cetacea.*

The anatomical and palæontological evidence for the association of the Ruminantia with other hoofed quadrupeds having the toes in equal number, in one natural order, called *Artiodactyla,* and for the grouping together of other hoofed animals with the toes in unequal number, in a second order called *Perissodactyla,* is given in the Quarterly Journal of the Geological Society for November 1847.

In regard to that family of Quadrumanous Mammalia which approach most nearly to Man, much obscurity prevailed at the close of Cuvier's labours. That great naturalist places the Orang-utan at the head of the order, from being acquainted with only the immature condition of the Chimpanzee. The knowledge of the osteological and dental characters of the *adults* of both forms, of their true facial angle and cerebral capacity, were first made known in Professor Owen's memoirs printed in the Zoological Society's Transactions; and here most of those characters which were supposed to approximate these animals most nearly to Man, are shown to be transitory, and peculiar to the young state of the animal with deciduous teeth.

In a second memoir in the second volume of the Zoological Transactions, Professor Owen gives the requisite details of the change of dentition, and describes a second species of Orang from Borneo (*Pithecus morio*). In a third memoir the cranial and dental characters of a second species of Chimpanzee (*Troglodytes gorilla*), of formidable strength and stature, discovered by Dr. Savage, are detailed in the third volume of the Zoological Transactions, to which Professor Owen has since added two memoirs descriptive of the entire skeleton of the *Troglodytes gorilla,* and the relative capacities of the cranium of the Orangs, Chimpanzees, and the different varieties of the human race.

With regard to the class of Birds, I may refer to Professor Owen's monograph on the Anatomy of the Toucan in Mr. Gould's works on the Rhamphastidæ; to his memoirs on the Anatomy of the Hornbill in the first volume of the Transactions of the Zoological Society; and to two elaborate monographs on the Anatomy of the *Apteryx Australis,* in the same Transactions.

The comparison of the organization of the latter remarkable species with that of the larger struthious birds, and, above all, the accessions to the same wingless order which we owe to Professor Owen's memoirs on the fossil remains of the *Dinornis* and *Palapteryx* obtained from the Islands of New Zealand, supplied him with the requisite grounds

for separating from the Grallæ of the Cuvierian system, the species that therein form the family '*Brevipennes,*' and in raising them to the rank of an order. This and other modifications of the Cuvierian classification of birds, and an inquiry into the grounds for a binary division of the class according to the condition of the newly-hatched young, *e. g.* into *Aves altrices* and *Aves præcoces*, will be found in Professor Owen's article *Aves* in the Cyclopædia of Anatomy and Physiology. Perhaps none of Professor Owen's researches on Fossil Remains have excited more general interest than those to which we are indebted for a knowledge of the gigantic Struthious Birds of New Zealand, the first paper on which is to be found in the third volume of the Transactions of the Zoological Society. I cannot avoid quoting on this subject the words of a distinguished geologist, a Fellow of the Society, in the 4th volume of the Quarterly Journal of the Geological Society.

"The first relic of this kind was made known to European naturalists by Professor Owen, in 1839. It consisted of the shaft of a femur or thigh-bone, but a few inches long, and with both its extremities wanting; and this fragment so much resembled in its general appearance the marrow-bone of an ox, as actually to have been regarded as such by more than one eminent naturalist of this metropolis. And if I were required to select from the numerous and important inductions of palæontology, the one which of all others presents the most striking and triumphant instance of the sagacious application of the principles of the correlation of organic structure enunciated by the illustrious Cuvier,—the one that may be regarded as the *experimentum crucis* of the Cuvierian philosophy,—I would unhesitatingly adduce the interpretation of this fragment of bone. I know not among all the marvels which palæontology has revealed to us, a more brilliant example of successful philosophical induction—the felicitous prediction of genius enlightened by profound scientific knowledge.

"The specimen was put into Professor Owen's hands for examination, and from this mere fragment, the Hunterian Professor arrived at the conclusion, 'that there existed, and perhaps still exists in those distant islands, a race of struthious birds of larger and more colossal stature than the Ostrich or any other known species.' This inference was based on the peculiar character of the cancellated structure of the bone, which differs from that of mammalia, and most closely resembles that of the Ostrich. And so confident was Professor Owen of the soundness of his inductions, that he boldly added, 'so far as my skill in interpreting an osseous fragment may be credited, I am willing to risk the reputation for it on this statement;' and he further remarks, 'The discovery of a relic of a large struthious bird in New Zealand is one of peculiar interest, on account of the remarkable character of the existing fauna of those islands, which still includes one of the most extraordinary and anomalous genera of the struthious order, the *Apteryx*; and because of the close analogy which the event indicated by the present relic offers to the extinction of the Dodo of the island of the Mauritius. So

far as a judgement can be formed of a single fragment, it seems probable that the colossal bird of New Zealand, if it prove to be extinct, presented proportions more nearly resembling those of the Dodo, than of any of the existing *Struthionidæ*.' In 1843 the correctness of these views was confirmed in every essential particular by a large collection of bones obtained by the Rev. W. Williams and transmitted to the Dean of Westminster; and still further corroborated by another interesting series brought to England in 1846 by Percy Earl, Esq."

It would be too long a trespass on your time to cite even the titles of the numerous Papers, Reports and Works in which the results of Professor Owen's researches in the field of Palæontology are recorded; and I am forced to pass with only an allusion, the numerous cases in which a fragment of a tooth has enabled him to decide the affinities of the animal to which it belonged, and to render the fragmentary remains of bones the means of determining the forms and relations of their former possessors. I may just enumerate as examples in illustration of the successful extent to which this principle of investigation was carried, its application to the Toxodon, the Mylodon, the Schidotherion, the Glyptodon, and many others.

This application of Comparative Anatomy to the right interpretation of the fragmentary remains of lost forms of animal life, is the last and perhaps the highest power which the cultivator of that science gains as such. It began now to be applied in a systematic manner by Professor Owen to the elucidation of the ancient zoology of this island. His first Report 'On British Fossil Reptiles,' was communicated to the British Association in 1839, the second and concluding Report on the same subject in 1842.

Subsequent researches on the extinct animals of the same class have been communicated in the Memoirs printed in the Transactions of the Geological Society, amongst which we may notice that on the Dicynodont Reptiles of South Africa; a Memoir on the Rhynchosaurus in the Transactions of the Cambridge Philosophical Society; and in Monographs contributed to the publications of the Palæontological Society. The matter of these Monographs and of the Reports has been methodized into a systematic 'History of British Fossil Reptiles,' now in course of publication, of which five Parts, each illustrated by twenty quarto or folio plates, have appeared.

In 1842 Professor Owen communicated his first Report 'On British Fossil Mammalia' to the British Association; and, in 1843, his second and concluding Report on the same class of extinct animals.

Both these and the preceding Reports on the Fossil Reptilia were drawn up at the instance of the British Association for the Advancement of Science, and the researches they necessitated were carried on chiefly by aid of grants from that body.

In the illustrated 'History of British Fossil Mammalia and Birds,' published in 1846, Professor Owen developes his generalization as to conformity of Geographical Distribution in the extinct and existing

forms of Mammalia, which he had progressively worked out in previous palæontological writings.

As examples of Anatomical Monographs, I may refer to the memoir on the *Lepidosiren annectens*, in the Linnæan Transactions for 1839; 'On the Anatomy of the Rhinoceros,' in the Zoological Transactions, and to the Papers 'On the Eustachian Canals in the Crocodile,' 'On the Carapace and Plastrum of the Chelonia,' 'On the Dentition of the Phacochœrus or Wart-Hog,' and 'On the Exogenous Processes of Vertebræ' in the Philosophical Transactions.

The value of microscopical research in comparative anatomy has been already alluded to, and a fresh instance of its importance is given in the elaborate researches on the subject of the Teeth, the first results of which were communicated in a report to the British Association at the Meeting at Newcastle in 1838, and they were ultimately embodied in the great work entitled 'Odontography,' comprising one volume of text and an atlas of 168 plates, in which the diversified modifications of the dental tissues in all classes possessing teeth are fully illustrated.

The minute structure of scales and other dermal appendages of Fishes has been studied microscopically by Professor Owen, who was led by the phenomena he observed to oppose the views of the development of scales by excretion, which M. Agassiz had contended for; and he demonstrates the close analogy which exists between the dermal bony tubercles and spines of the cartilaginous fishes and their teeth.

Professor Owen's views have been confirmed, the analogy extended, and a variety of beautiful modifications of tooth-like structure demonstrated by Dr. Williamson in his papers recently published in onr Transactions.

Of the application of the microscope by Professor Owen to the solution of some of the mysterious problems of generation, examples will be found in his 'Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals,' published in 1843; in those 'On the Generation and Development of Animals,' published in 1849–50; and in his work 'On Parthenogenesis, or the successive production of Procreating Individuals from a single Ovum,' also published in 1849. In the latter work Professor Owen shows the intent of the 'cleavage process,' as it has been called, to be that by which the spermatic principle is distributed throughout the germ-mass: and he there points out the consequent relation of such inherited subdivision of the spermatic principle to future developments of embryos in virgin parents. As propounded in this work, the theory became capable of application to many other cases besides that to which it was first applied.

The progress of Natural History has added many analogous instances of virgin-generation to that of the *Aphides*. In all these Professor Owen calls attention to the proposition of the primary cell-structure of the impregnated germ, which is retained in the procreative larvæ. Dr. Steenstrup has very ably and very ingeniously generalized the phenomena in question in his well-known essay on 'Alternation of Generations.'

The progress of all sciences is a perpetual struggle after generalizations of a higher and higher order. Anatomy and physiology, so actively cultivated in the time of Cuvier, had afforded at the latter end of his career, glimpses of generalizations, which, under the vague terms of "unity of organization," became subjects of sharp controversy. The idea, so expressed, had two applications,—*one*, to the analogies which exist between the permanent organization of the lower animals, and certain transitory states of the higher species; *the other*, to the correspondences traceable between the parts composing the organization of different species.

With reference to the first of these applications, I cannot do better than quote the author's own account of his conclusions, as given in the last lecture of his course on the Invertebrate Animals, published in 1843.

"The extent to which the resemblance, expressed by the term 'Unity of Organization,' may be traced between the higher and lower organized animals, bears an inverse ratio to their approximation to maturity. All animals resemble each other at the earliest period of their development, which commences with the manifestation of the assimilative and fissiparous properties of the polygastric animalcule: the potential germ of the mammal can be compared, in form and vital actions, with the Monad alone, and, at this period, unity of organization may be predicated of the two extremes of the Animal Kingdom. The germ of the Polype pushes the resemblance farther, and acquires the locomotive organs of the Monad—the superficial vibratile cilia—before it takes on its special radiated type. The Acalephe passes through both the Infusorial and Polype stages, and propagates by gemmation, as well as spontaneous fission, before it acquires its mature form and sexual organs. The fulness of the unity of organization which prevails through the Polypes and larval Acalephes, is diminished as the latter acquire maturity and assume their special form.

"There is only one animal form which is either permanently or transitorily represented throughout the Animal Kingdom,—it is that of the infusorial Monad.

"Other forms are represented less exclusively in the development of the Animal Kingdom, and may be regarded as secondary forms. These are, the Polype, the Worm, the Tunicary, and the Lamprey; they are secondary in relation to the Animal Kingdom at large, but are primary in respect of the primary divisions or sub-kingdoms.

"Thus the Radiata, after having passed through the Monad stage, enter that of the Polype: many there find their final development; others proceed to be metamorphosed into the Acalephe or the Echinoderm.

"All the *Articulata*, at an early stage of their development, assume the form or condition of the apodal and acephalous worm; some find their mature development at that stage, as the Entozoa; others proceed to acquire annulations; a head; rudimental feet; jointed feet, and finally, wings: radiating in various directions and degrees from the primary or fundamental form of their sub-kingdom.

"The *Mollusca* pass from the condition of the ciliated Monad to that of the shell-less Acephalan, and in like manner either remain to work out the perfections of that stage, or diverge to achieve the development of shells, of a head, of a ventral foot, or of cephalic arms.

"The vertebrated ovum having manifested its monadiform relations by the spontaneous fission, growth, and multiplication of the primordial nucleated cells, next assumes, by their metamorphosis and primary arrangement, the form and condition of the finless cartilaginous fish, from which fundamental form development radiates in as many and diversified directions and extents, and attains more extraordinary heights of complication and perfection than any of the lower secondary types appear to be susceptible of."

To the second application of the principle, I must more particularly refer, as the subject on which perhaps Professor Owen's investigations have been more fully and elaborately and systematically carried out, and have exercised a more important and universal influence on these sciences than any other,—I mean the doctrine of Homologies, or the correspondency of parts and of plan in the construction of animals. This had been the subject of close and sharp discussions in the Academy of Sciences between Cuvier and Geoffroy St. Hilaire, which are summed up by the latter in the 'Principes de Philosophie Zoologique,' published in 1830; and it can be no matter of surprise, that with an antagonist so strong in his well-founded reputation, as a great master in science, and so skilful in applying the weapons of a severe and sarcastic logic, Geoffroy St. Hilaire should have failed to impress the physiological world with those views which Cuvier objected to, as being based upon *à priori* speculation.

The effect of these discussions may be traced in most of the ablest works on Anatomy and Physiology which subsequently appeared, as, *e. g.* those by Prof. John Müller, Prof. Wagner, Milne-Edwards, Siebold and Stannius; and in the 'Outline of the Animal Kingdom' and 'Manual of Comparative Anatomy' by Professor Jones of King's College, London. By all these authors, the principle of Unity of Organization, as it has been attempted to be illustrated and applied by Geoffroy St. Hilaire, and by the German Anatomists of the Trancendental School of Schelling, is tacitly or avowedly abandoned. By M. Agassiz it was directly opposed.

Nevertheless, the question whether the principle of a common pattern, or the principle of final causes, or, as Cuvier called it, 'conditions of existence,'—I say, which of these two principles, or in what degree both have governed the development of organization of animals—the greatest question which can occupy the philosophical anatomist—was still far from having been satisfactorily decided.

It enforced itself upon the most serious consideration of Professor Owen, when he was called upon to prepare the Catalogue of the Osteological Collections in the Museum of the Royal College of Surgeons: and the results of this consideration were promulgated

in his Lectures on Comparative Osteology, given in the Theatre of the College.

What those results are, may now be studied in his 'Report on the Homological Relations of the Skeleton,' submitted to the British Association at Southampton in 1846, in his 'Lectures on the Vertebrate Animals,' 1846, and in his works entitled 'On the Archetype and Homologies of the Vertebrate Skeleton,' 1848, and 'On the Nature of Limbs,' in 1849.

It does not become me, if even time permitted, to enter upon an analysis of these works. I believe them to be well known to all my anatomical and physiological hearers, and that the doctrines they contain, new rather than revived—new at least, in the best sense, as being the results of strict induction,—have been generally received.

In the comprehensive 'Principles of Physiology, General and Comparative,' by Dr. Carpenter, the first systematic work on the subject which has appeared in our language since the promulgation of Professor Owen's views, that author adopts the philosophy of the skeleton therein set forth, borrows the illustrations of the Vertebrate Archetype, and characterizes the works from which he quotes as "examples of that rare combination of logical appreciation of facts, with originality in the conception of ideas, which distinguishes the true philosopher from the rash speculator on the one hand, and from the mere plodding observer on the other."

Sir Charles Lyell, in his last 'Anniversary Address' to the Geological Society, speaks of the same works as being "distinguished by grand and comprehensive views in regard to the relations of different parts of the vertebrate creation to each other."

Dr. Carus, in the attempt to follow out the homologies of the muscles on the principles laid down by Professor Owen, in his 'Lectures on the Vertebrate Animals' for the application of his philosophy to that system, acknowledges it as "indicating the only true way to the comprehension of a scientific myology."

The great aim of Professor Owen's works on Homological Anatomy, appears to be to put an end to the old controversy so long maintained, on the assumption that a special adaptation of parts was incompatible with a common type of construction. Having, after long pains-taking researches, arrived at a clear conception of the archetypal plan of the vertebrate structures, he associates that idea with as clear a recognition of the teleological signification of the great principle as our finite capacities are able to attain to. "For it is certain," writes Professor Owen, "that in instances where the analogy of a machine fails to explain the structure of an organ, such structure does not exist in vain, if its true comprehension lead rational and responsible beings to a better conception of their own origin and Creator." Thus, far from giving support to Transmutational or Pantheistic notions, the conclusions of the Homologist being based on a rigorous deduction from carefully observed facts, furnish new arguments in support of the highest truths.

Our allotted time has prevented me from entering, to any extent,

into many of the subjects which have been elucidated by the researches of Professor Owen. But I think the sketch which I have been able to lay before you, presents such an amount of labour, guided by genius, and rewarded by important results, as is scarcely exampled in the history of natural science, and shows how justly the Council have decided in awarding the highest honour the Royal Society can bestow, on Professor Owen.

The Royal Medal in the department of Astronomy having been awarded to the President, Colonel Sabine addressed his Lordship as follows:—

Lord Rosse,

It falls to me to present to your Lordship the Royal Medal in the department of Astronomy, merited by researches not more remarkable from the universal interest which they inspire, or the brilliant and unexpected results which crown them, than from the rare combination of industry, patience, inventive genius, and scientific power to which they owe their success. You have reopened a field of investigation which seemed almost exhausted by two of the most illustrious observers of this or any age; and have added another proof of the great truth, that wide as is the range of human intellect, the wonders of Divine Wisdom lengthen out without limit beyond that range on every side. Even in your first memoir 'On Nebulæ' (Philosophical Transactions, 1844), it was evident that the resolution of a large proportion of those mysterious forms into clusters might be expected, and that even with the optical power of a three-feet reflector their configurations would assume new aspects:—and these anticipations have been fully realized by the gigantic instrument whose first fruits you have presented to the Royal Society. Without attempting to analyse your second memoir, which must be familiar to all who think in this department of Astronomy, I would notice as most prominent:—

1. The resolution of a great number of Nebulæ not previously resolved; and the discovery, that in most of these the stars are neither uniform in magnitude nor distribution. In the most remarkable, the great Nebula of Orion, its components are very minute, but crowded in knots, giving it a mottled appearance; while in others (the Dumb bell for example), such small stars envelope an assemblage of others of greater magnitude. Thus, the tokens of resolvability, which the sagacity of Sir John Herschel had discerned, are verified, and their application warranted to such Nebulæ as resist even the six-foot reflector. There is no line of demarcation between Clusters and Nebulæ.

2. Nor have the symmetric forms and exact circular or elliptic outlines which many of the Nebulæ were supposed to possess, when seen with instruments too weak to bring out many of their details, any existence: the hypothesis which infers from such appearances their gradual consolidation under the influence of rotation and gravity

has therefore no foundation, and their actual arrangement rather indicates the operation of causes presenting a marked difference from those which act in our planetary system.

3. Your Lordship's researches have disclosed an arrangement even more astonishing and more suggestive than any which had been previously ascribed to Nebulæ;—that spiral conformation which prevails in so many instances, occasionally displayed with all the graceful precision of a geometric curve, but most frequently seen obliquely, and causing the appearance of curved luminous or dark bands. It is found connected with single or multiple centres, clusters or stars; with rings probably of stars, and even may be traced in ordinary clusters. What are the conditions of which it is the result? If the Astronomer finds it hard to conceive the laws which can maintain the permanence of a uniform globular cluster, how much enhanced is the difficulty, when to the perturbing forces which exist in that simplest case are added others, such as the vortex-like character of these marvellous forms!

All this would be much had it been achieved by one who sought the means of fathoming the depths of the sky in the workshops of Munich, Paris, or London; but it is much more when he not merely uses, but has himself created that transcendent explorer, whose possible existence would a few years since have been regarded with incredulity. You, my Lord, have not only overcome the difficulties which had deterred professional opticians, by a course of costly experiments continued with consummate skill and science for many years, but have made it a special object to communicate the knowledge so laboriously obtained for the public use. The Council has awarded to your Lordship the Medal which Her Majesty, our Patroness, has graciously placed at the disposal of the Royal Society, as the highest testimonial which, as your Lordship is one of the Council, it is in their power to offer; but we are sure that you would deem it even a higher one, should we be able to establish in another region an observer, who would imitate the course of discovery on which you have so gloriously entered; and which it is the earnest desire of every friend of science that you may long be permitted to pursue.

The President then addressed Mr. Newport.

Mr. Newport,

I am most happy that the important services you have rendered to Physiological Science have been again rewarded, and that the pleasing duty has devolved upon me of placing in your hands, a Royal Medal, for your paper 'On the Impregnation of the Ovum in the Amphibia,' First Series, published in the Philosophical Transactions for 1851.

In dealing with that obscure and difficult subject, you have proceeded cautiously, and in a true philosophic spirit, deriving your information from experiments, ingeniously contrived, and ably exe-

cuted. To have obtained some new and valuable results in such an inquiry, was certainly an important achievement; the value, however, has been enhanced by your deductions, which are cautious and wary, as well as interesting and new. Allow me to express a hope that your success may be a stimulus to farther efforts, to be rewarded by discoveries honourable to you, to this Society, and to English Science.

The President then called upon Mr. Christie to read the biographical notices of some of the deceased Members, which he handed to him.

By the death of HENRY CHRISTIAN SCHUMACHER, on the 28th of last December, at the age of 70 years, the Society has lost a valuable and amiable associate; one who became the personal friend of all the scientific men with whom he had anything to do. He was born in Holstein in 1780, of highly respectable parents; but losing his father early, he was placed under the care of the Pastor Dörfer, until his mother removed to Altona for the sake of educating her two sons. The Gymnasium to which he was now sent was fortunately presided over by Jacob Struve, father of the zealous Pulkova astronomer, who earnestly instructed his promising pupil both in classics and mathematics; but being of a delicate constitution, young Schumacher sought sedentary amusements, and was indulged in his partiality for mechanics, which he specially applied himself to in order to satisfy his mind on the action of various instruments connected with astronomy. Having gained the highest honours of the Gymnasium, he now thought of studying the law, for which purpose he repaired to Kiel; but in 1804, being recommended as tutor to a noble family residing near Dorpat, his passion for mathematics returned under the encouragement of Professor Pfaff, of the University of that city, whom he assisted during the short duration of a work called the 'Astronomische Beyträge.' In 1807, he took his Doctor's Degree, and returned to Altona, two years after which he obtained the Danish Government's permission to complete his studies at Göttingen under the celebrated Gaüss, whose friendship he enjoyed to his death. But the year following he was appointed Professor of Astronomy at Copenhagen; and in 1813, he had the satisfaction of having an Observatory placed under his direction at Mannheim. In a couple of years more, however, he was promoted to that of Copenhagen, to fill the vacancy occasioned by the death of Professor Bugge.

Schumacher's active intelligence now led him to look around, in order to ascertain what was transacting in other countries;—and he accordingly obtained permission to visit France and England in 1819, with his intimate friend Repsold of Hamburg, the eminent mechanician; and benefiting by our great trigonometrical survey, even to obtaining the loan of Ramsden's Zenith-sector from the English Government, he commenced measuring the Danish arc of the meridian, from Lauenburg in Holstein northwards. This service requi-

ring his frequent absence from Copenhagen, he was allowed to establish himself at Altona, and there build an Observatory and Museum of astronomical and geodesical instruments of all countries. Although immersed in these duties, as well as in making various chronometric journeys for the express purpose of differentiating and fixing longitudes, he steadily kept an eye on the scientific operations of the rest of Europe; and he visited at successive times his correspondents at Paris, Munich, Berlin, Vienna and Pulkova; besides a frequent trip to Bremen to consult the truly philosophical Dr. Olbers, and an annual journey to Copenhagen to report progress to his warm patron Frederick VI., and the equally friendly Christian VIII.

At the age of 32, Schumacher married with judgement a lady of good family, Christine Madelaine Schoon, who survives him; and their family consists of three daughters and two sons, one of whom, Richard, has already distinguished himself in astronomy.

Professor Schumacher was polished in his manners, cheerful in conversation, and could speak in German, Danish, Russian, French, English and Italian. He was fond of drawing, a great adept in the game of chess, remarkably punctual in all his occupations, unwearied in his application to business, neat in his computations, and orderly with his instruments and books; while a refined mind pervaded all his actions. The benefits consequent on these habits spread wonderfully over his long succession of assistants, to whom his time and advice were never denied, and of whom no fewer than sixteen now hold ostensible scientific posts. During the late political struggle, the principal scientific societies of Europe entreated the Danish Government to continue the Altona Observatory, as an establishment of paramount value to the world; and so it appears to have been viewed by the contending parties, for they both offered him their support. He died, however, before that very lamentable war had ceased; and is succeeded by his worthy pupil Dr. Petersen. But it will be difficult to replace such a fulcrum of scientific intercourse as Schumacher proved to be; especially in establishing and continuing through twenty-seven years, that cosmopolitan bond of intellectual union the 'Astronomische Nachrichten,' of which, from his practical and theoretical knowledge, and his philological attainments, he was perhaps the most effective editor which Europe could have produced. Besides this he published 'Géometrie der Stelling von Carnot,' 'Sammlüng von Hülfstafeln,' 'Astronomische Hülfstafeln,' 'Astronomische Abhandlungen,' and other works.

Schumacher had been elected into most of the Scientific Societies of the world, being thus honoured in America, Denmark, France, Germany, Great Britain, Italy, Russia, and Sweden; and his date of election as a Foreign Member of this Society is 1821. He also received decorations of knighthood from Prussia, Russia, Sweden, France and Belgium; on which last order being conferred on him, that of Leopold, for his connexion with the documents furnished by the Danish Government, he wrote to explain that the only participation he had with Major Olsen's surveys, was their resting on his triangulation. But his scrupulous delicacy was overcome when he was informed, in

reply, that it was rather the opportunity of honouring him that had been seized, for that the order had also been conferred on the Major. The latest favour he received from his own Sovereign, Christian the VIIIth, was the Grand Cross of the Order of Dannebrog, and Dannebrog's Man; which distinction was accompanied with a very complimentary letter, in which the monarch declared his esteem for literary and scientific merit.

When Lord Northampton's decease was announced in a literary journal (the Athenæum*), it was remarked, that " though it sounds like a truism to say that the union of science and station and exalted character in the same individual seldom fails to command esteem,"—this truism was precisely the tribute which suggested itself in this case. And it was no doubt true that esteem, produced by the kindness, courtesy, truthfulness, fairness, and good sense of his character, which qualities were made conspicuous by his rank, and his frequent appearance in public, was the universal sentiment entertained towards him; yet no one who knew him well could fail also to admire his fine intellect, richly cultured mind, and varied knowledge, both of literature, science and art. It was this union of qualities, rarely found in the same person, which especially recommended him to your choice for ten successive years as President, and endeared him to this Society, and to his extended circle of friends and acquaintances.

Spencer Joshua Alwyne Compton, Marquis and Earl of the County of Northampton, was born on the 2nd of January 1790. He was educated at Trinity College, Cambridge, and manifested a love for literature and the classics, which he cultivated with an assiduity not always found in men of his rank. He there not only laid the foundation of the accomplishments and information which distinguished him in after-life, but formed many friendships with eminent men, now ornaments of this Society; and these friendships were not only continued, but increased and strengthened during the remainder of his life.

On quitting the University, Lord Compton was returned to Parliament as Member for the town of Northampton. Spencer Percival, the Prime Minister of the day, was his near relative, and thus a political career of eminence was within his grasp. But a sense of duty led him to join the opposition ranks; and being defeated at the next Parliamentary election, he retired from the political field of the House of Commons. Lord Compton associated himself at this period with Wilberforce, and the noble and excellent men who devoted themselves to the cause of Africa. The same associations connected him with Sir James Mackintosh as a criminal law reformer.

In 1815 he married Miss M'Cleod Clephane, daughter and heiress of General Clephane, a lady whose native and original genius had been matured by the most careful cultivation. She was at an early age a favourite of Walter Scott, who was delighted with her genius

* The biographical notice in the Athenæum which was thus prefaced has been freely used on the present occasion.

and her love of poetry. Among many and consummate accomplishments, her poetical talent was perhaps her most remarkable gift. Lord and Lady Compton travelled in Italy, and the houses which they successively occupied in that beautiful land were the centres of attraction for refined and intelligent travellers. Lord Compton also interfered actively and effectively on behalf of some of the unfortunate Italians who fell under the severe measures of authority both in Lombardy and at Naples. For years Italy was the favourite residence of this excellent English family, and for years they might be pointed out to foreigners with pride as representatives of the British Aristocracy. But the greatest of all domestic calamities was impending. In 1830 Lord and Lady Northampton, who had succeeded to the honours of the family in 1828, were residing at Rome, when by a most sudden and overwhelming calamity he found himself a widower. Lord Northampton removed his family to England at once, and at his noble seat of Castle Ashby he devoted himself to the education of his children, and the cultivation of literature and science.

In 1830 he joined this Society, and his connexion with it is undoubtedly one of the most remarkable features of his life. When the Duke of Sussex resigned the Presidency, the feeling was general that he was a fit person to succeed his Royal Highness. Though his scientific attainments might not be profound, he was an ardent lover of science, and testified his love by gathering around him all those who had distinguished themselves in its various departments. He was elected President in November 1838, and annually re-elected until November 1848. During his term of office, with few exceptions, he was always in his place at the Meetings of the Council, and his attention to the business that came on, fairness, and good humour, were worthy of all praise.

The Soirées which he gave in his capacity of President were attended by all the rank and science in the country; and their influence upon the world generally was of the happiest nature. Perhaps we may regard the labours of men of science respecting magnetism, and especially the establishment of a connected system of magnetic observations over the greater part of the earth's surface, as one of the principal subjects which employed the Royal Society during his Presidentship; and the applications of Lord Northampton to the government on this subject were assiduously made and were always favourably received.

Lord Northampton had paid especial attention to geology and mineralogy. He was the discoverer of a new mineral in the lava of Vesuvius, which was after him named Comptonite. He communicated to the Geological Society a description of the Basaltic Rocks of the Isle of Mull, with which place, so remarkable both by its scenery and its geology, he was especially connected: and at a later period (in 1838) he made to the Geological Society another communication on Spirolites in Chalk and Chalk flints, the objects so distinguished being extremely minute organic fossils.

The British Association for the Advancement of Science was one

of the fields in which Lord Northampton's scientific zeal and knowledge, and the admirable qualities which enabled him to conciliate opposing views while he maintained the authority of rule and discipline, were seen to eminent advantage. He showed himself ready, upon multiplied occasions, and on the most sudden and unexpected emergencies, to give to that Association the benefit of his time, his talents, and his energy. This is not the occasion to enumerate his great services to that body, but the cause of the promotion of science which we have in common with that body, and the general sympathy prevailing between this Society and the Association, as shown by the number of our members who take a leading part in the Meetings of the Association, have made it allowable to say so much on a subject which Lord Northampton had very much at heart, and to which on several occasions he referred in his addresses.

Archæology was another of Lord Northampton's studies, to which he devoted much time. He was in the habit of travelling, both in England and in foreign countries, with the purpose of seeing, drawing, and annotating the most remarkable specimens of ancient, and especially of Gothic architecture. He accumulated in this way very considerable stores of drawings and notes, some of the results of which he circulated among his friends in various forms. These pursuits led him to take a lively interest in the Archæological Association, which was established on a plan similar to the British Association, and which afterwards separated into two bodies, to one of which, the Archæological Institute, he continued to render great services at its annual meetings.

Lord Northampton's acquaintance with literature was various and extensive, and some poems of his printed in a publication called the 'Tribute,' under circumstances which marked his considerate benevolence, are graceful evidences of his poetical feeling. His literary merits were recognised by his election to succeed Mr. Hallam as President of the Royal Society of Literature, which office he held at the time of his death. Lord Northampton was also a Trustee of the British Museum.

Besides the writings and publications which we have already noticed, he published at the time of the discussions respecting the Reform Bill, a pamphlet written in a tone of philosophical moderation and political wisdom, as probably even those would allow who did not agree with his views. The work was a letter addressed to Mr. Spring Rice, now Lord Monteagle, one of his oldest and nearest friends; and recommended a change in the law so that seats in the House of Commons should not be vacated by acceptance of office. The measure was however rejected by Parliament.

Lord Northampton's health was habitually delicate. For many years he had been afflicted by a spitting of blood; and on more than one occasion he had been obliged to retire from active life and England to repose in southern climates. His last tour abroad was made in company with his son-in-law, the late Viscount Alford, who was recommended to spend a winter in the east for the benefit of

his health. The change did not produce the hoped result. Lord Alford gradually sank; and his death proved too great a shock to the naturally sensitive temperament of Lord Northampton. He expired only a few days after the decease of his son-in-law, at his seat of Castle Ashby, on the 16th of January, surrounded by all the members of his family except a son, who was with his regiment in India.

It is difficult to abstain from dwelling longer on Lord Northampton's admirable gifts and accomplishments, and still more, on his virtues. He was full of kindness and benevolence for all who came under his notice, and seemed to be absolutely incapable of injustice or unfairness; and though a most clear-sighted judge of intellectual, scientific and artistical excellence, was with difficulty, if at all, moved to harshness towards shallow and petulant pretensions. He was zealous for the promotion of art as well as science in his native country; and even in the last days of his life his thoughts and his pen were engaged on a plan connected with that object.

Thomas Galloway, Esq., late a Member of the Council of this Society, died of disease of the heart on the 1st of November, at his residence in Torrington Square, and was buried at Kensal Green.

Mr. Galloway was born in the Upper Ward of Lanarkshire, on the 26th of February, 1796, his father being a farmer, and his grandfather a mechanical engineer. He was educated at the parish school of his neighbourhood, at the Academy of New Lanark, and the University of Edinburgh; at which last he entered in November 1812, and continued there eight sessions. At first he was mostly devoted to classical studies, but turned to mathematics as a special pursuit by the encouragement and teaching of the late Professor Wallace.

His first publication, probably, was the article on Pendulums in Brewster's Cyclopædia, about 1821; and he afterwards wrote various scientific articles in Leybourn's Repository and the Encyclopædia Britannica, and he moreover published a special work on 'Probabilities.' In the year 1823, he was elected Teacher in the Royal Military College at Sandhurst, where his accuracy of knowledge and business-like habits rendered him both efficient and popular.

In 1831, he married a daughter of his friend Professor Wallace; and in November 1832, he was selected for further consideration out of all the Candidates for the Chair of Natural Philosophy in the University of Edinburgh, by the Town Council, with Sir David Brewster and Professor Forbes; when the latter was elected. But in the year following he was called to undertake the important office of Register, that is, in modern language, Actuary of the Amicable Life Assurance Office, the oldest Institution of the kind; the duties of which he ably conducted to the day of his regretted death.

The gentlemen present will remember that Mr. Galloway was the author of an excellent paper on the 'Proper Motion of the

Solar System,' which was published in the Philosophical Transactions for 1847; and for which the Royal Medal was awarded to him. This interesting discussion was occasioned by Professor Argelander's having published an opinion on the probable situation in space towards which the sun is at present advancing. The inference thus drawn, was from an investigation of the proper motions of stars in the northern hemisphere; and Mr. Galloway was induced to inquire into the same question by a direct comparison of the positions of stars determined by Lacaille, with the recent Catalogues of Messrs. Johnson and Henderson, in the Southern hemisphere;—a considerable number of which appear to have very appreciable proper motions. This argument he entered into thoroughly, and with a neat and expressive mathematical computation; but he concluded, that the data were not yet sufficiently numerous to warrant any speculation with respect to the nature of the path which the sun describes in space.

Mr. Galloway was also the author of two valuable papers which were communicated to the Royal Astronomical Society, of which he was for some years a distinguished Secretary. The first of these, on the Application of the Method of Least Squares to the determination of the most probable errors of observation in a portion of the Ordnance Survey of England, was published in the Astronomical Memoirs for 1846: the second was on the present state of our knowledge relative to Shooting Stars, and on the determinations of differences of longitude from observations of those meteors,—a subject which he treated with his usual research and accuracy.

On the motion of Sir R. H. Inglis, Bart., seconded by Sir R. I. Murchison, the best thanks of the Society were tendered to the President for his excellent Address, and his Lordship was requested to permit the same to be printed and circulated among the Society.

The Statutes relating to the election of Officers and Council having been read, and Mr. G. R. Porter and Mr. S. P. Pratt having, with the consent of the Society, been nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentlemen were reported duly elected Officers and Council for the following year:—

President—The Earl of Rosse.

Treasurer—Lieut.-Colonel Sabine, R.A.

Secretaries— { Samuel Hunter Christie, Esq.
Thomas Bell, Esq.

Foreign Secretary—Captain W. H. Smyth, R.N.

Other Members of the Council.—W. Bowman, Esq.; B. C. Brodie, Esq.; Professor Challis; Dr. W. Clark; Dr. Daubeny; Sir

Philip Egerton, Bart.; The Dean of Ely; J. P. Gassiot, Esq.; Dr. Marshall Hall; Sir John Herschel, Bart.; Professor W. H. Miller; Lieut.-Colonel Portlock, R. E.; Edward Solly, Esq.; W. Spence, Esq.; Dr. Wallich.

The thanks of the Society were given to the Scrutators for their trouble in examining the lists.

The following is a statement of the Receipts and Expenditure during the past year:—

Statement of the Receipts and Payments of the Royal Society between Nov. 30, 1850, and Dec. 1, 1851.

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance in the hands of the Treasurer at the last Audit ..				156	18	8
Weekly Contributions, at one shilling				41	12	0
Quarterly Contributions at £4				1076	0	0
15 Admission Fees				150	0	0
5 Compositions for Annual Payments at £60				300	0	0
7 Compositions for Annual Payments at £40				280	0	0
One year's rent of estate at Mablethorpe: due at Michaelmas 1850	110	0	0			
One year's Income Tax	3	1	0			
				106	19	0
One year's rent of estate at Acton: due at Michaelmas 1851	70	0	0			
One year's Income Tax	2	0	10			
				67	19	2
One year's Fee farm rent of lands in Sussex: due at Michaelmas 1851				19	4	0
One year's rent from Royal College of Physicians				3	0	0
Dividends on Stock:—						
One year's dividend on £14,000 Reduced 3 per cent. Annuities	420	0	0			
Less Income Tax	12	5	0			
				407	15	0
One year's dividend on £6985 3s. 8d. 3 per cent. Consols	209	6	6			
Less Income Tax	5	17	8			
				203	8	10
Carried forward..........				2812	16	8

				£	s.	d.
Brought forward..........				2812	16	8
One year's dividend on £3452 1*s*. 1*d*. 3 per cent. Consols, produce of sale of premises in Coleman Street........................	103	11	2			
Less Income Tax	3	0	4			
				100	10	10
Donation Fund.						
One year's dividend on £5331 10*s*. 8*d*. Consols	159	18	6			
Less Income Tax	4	13	0			
				155	5	6
Rumford Fund.						
One year's dividend on £2430 12*s*. 5*d*. Consols	72	17	9			
Less Income Tax	2	1	9			
				70	16	0
Fairchild Fund.						
One year's dividend on £100 New South Sea Annuities				3	0	0
Bakerian Lecture and Copley Medal Fund.						
One year's dividend on £366 16*s*. 1*d*. New South Sea Annuities	10	18	0			
Less Income Tax	0	6	2			
				10	11	10
Wintringham Fund.						
One year's dividend on £1200 Consols	36	0	0			
Less Income Tax	1	1	0			
				34	19	0
Miscellaneous Receipts:—						
Sale of Philosophical Transactions, Abstracts of Papers, and Catalogues of the Royal Society's Library				438	17	5
One-half Expense Printing Colonel Sabine's Magnetical Papers, Nos. 7, 8 and 9, repaid by Government...............................				311	12	3
Total Receipts......				£3938	9	6

PAYMENTS.

	£	s.	d.
Fairchild Lecture.—The Rev. J. J. Ellis, for delivering the Fairchild Lecture for 1851	3	0	0
Bakerian Lecture.—Professor Faraday, for the Bakerian Lecture for 1851	4	0	0
Carried forward..........	7	0	0

	£	s.	d.	£	s.	d.
Brought forward..........				7	0	0
Salaries:—	£	s.	d.			
S. H. Christie, Esq., one year, as Secretary..	105	0	0			
Thomas Bell, Esq., one year, as Secretary ..	105	0	0			
Ditto for Index to Phil. Trans.	5	5	0			
Capt. Smyth, one year, as Foreign Secretary..	20	0	0			
Charles R. Weld, Esq., one year, as Assistant-Secretary	300	0	0			
Mr. White, one year, as Clerk	100	0	0			
Porter	40	0	0			
				675	5	0
Purchase of £606 1*s.* 2*d.* 3 per cent. Consols				600	0	0
Fire Insurance, on the Society's Property				45	1	6
Gratuity to Bank Clerks				1	1	0
Ditto to late Porter's Widow				25	0	0
Powers, Cleaning Rooms and Books				13	11	0
Bills:—						
Taylor:						
Printing the Phil. Trans., 1850, part 2 ..	351	4	0			
Ditto, 1851, part 1..................	367	5	9			
Ditto, Proceedings, Nos. 76—81; Circulars, Lists of Fellows, Ballot-lists, Statement of Payments, Minutes of Council; Government Grant Committee, Notices, &c. &c.	162	9	8			
				880	19	5
Basire:						
Printing Plates in Transactions, 1850, part 2	174	19	0			
Engraving, 1851, part 1..............	26	19	0			
Ditto, part 2	71	6	6			
				273	4	6
Dinkel:						
For Lithography	34	14	0			
Wing:						
For ditto	70	19	6			
F. Gyde:						
For Wood Engraving................	19	17	0			
				125	10	6
Bowles and Gardiner:						
Paper for the Phil. Trans., 1850, part 2,	158	8	0			
and 1851, part 1..................	143	11	0			
				301	19	0
Gyde:						
Boarding and Sewing 800 Parts of Phil. Trans., 1850, part 2	22	18	0			
Ditto, 1851, part 1..................	22	18	0			
Ditto, Extra binding	22	8	0			
				68	4	0
Carried forward..........				3016	15	11

				£	s.	d.
Brought forward..........				3016	15	11
Tuckett:						
Bookbinding	60	12	0			
Limbird:						
For Stationery	10	11	0			
Saunderson:						
For Shipping Expenses	21	7	6			
Brecknell and Turner:						
Candles, and Lamp Oil	41	6	0			
Arnold:						
For Coals	28	12	0			
Meredith:						
Mats, Brushes, Fire-wood, &c.	8	10	3			
Cubitt:						
For repairs and relaying Carpets, &c.......	26	18	0			
Slack:						
For Repairs	6	12	7			
Shoolbred:						
For Oil-cloth	4	7	0			
Charlton:						
For Cases and Shelves	20	7	3			
Sharpus:						
For China	3	5	3			
Humphries:						
For Livery	5	10	0			
Tea, Waiters, &c. at Ordinary Meetings	32	18	3			
Two Subscriptions over paid	8	0	0			
Draining Estate at Mablethorpe............	54	11	0			
Visiting ditto	4	14	6			
				338	2	7
Books purchased:						
Dulau and Co.: for Books	18	19	6			
Taylor: for ditto	33	17	8			
Gould: for ditto	17	17	0			
Second-hand ditto	29	19	0			
				100	13	2
Taxes:						
Land and Assessed Taxes	9	1	1			
Income Tax	4	19	2			
				14	0	3
Copley Fund:						
Mr. Wyon, for Medals				37	17	0
Donation Fund:						
Mr. Ronalds	100	0	0			
Mr. Horner	50	0	0			
				150	0	0
Carried forward..........				3657	8	11

				£	s.	d.
Carried forward				3657	8	11
Few and Co.:						
Law Expenses				8	6	10
Petty Charges:						
Postage and Carriage	34	13	2			
Expenses on Foreign Packets, &c.	16	16	7			
Stamps	2	12	0			
Charwoman's Wages	27	6	0			
Extra Cleaning	2	2	0			
Miscellaneous expenses	41	15	6			
				125	5	3
Balance in the hands of the Treasurer				147	8	6
Total				£3938	9	6

EDWARD SABINE, *Treasurer.*

December 1*st*, 1851.

Estates and Property of the Royal Society.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.). Rent £110 per annum.

Estate at Acton, Middlesex (33 acres). Rent £70 per annum.

Fee farm rent in Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per cent. Annuities.

£20,005 9*s*. 11*d*. Consolidated Bank Annuities.

£366 16*s*. 1*d*. New South Sea Annuities.

The Receipts during the past year, exclusive of the Balance, amounted to :—£3781 10*s*. 10*d*.

The Expenditure during the same period, exclusive of the sum of £600 0*s*. 0*d*. invested in the Funds, was :—£3191 1 0

The Balance in hand, now belonging to the Donation Fund, is £120 16*s*. 11*d*.

Ditto, ditto Wintringham Fund, is £198 16*s*. 3*d*.

Annual Contributions.

1830	£363	4	0
1831	286	0	0
1832	255	6	0
1833	283	7	6
1834	318	18	6
1835	346	12	6
1836	495	0	0
1837	531	0	0
1838	599	4	0
1839	666	16	0
1840	767	4	0
1841	815	12	0
1842	910	8	0
1843	933	16	0
1844	1025	16	0
1845	1010	0	0
1846	1074	0	0
1847	1116	8	0
1848	1122	16	0
1849	1130	16	0
1850	1146	4	0
1851	1117	12	0

The following table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s*. Annually.	Paying £4 Annually.	Total.
November 1850....	12	50	441	17	278	798
Since elected......	......	......	+9	......	+6	+15
Since compounded	......	......	+5	......	−5	....
Defaulters	......	......	......	......	−2	−2
Withdrawn	......	......	......	......	−3	−3
Since deceased	−1	−4	−18	−2	−6	−31
December 1, 1851..	11	46	437	15	268	777

December 11, 1851.

COLONEL SABINE, R.A., V.P. & Treas., in the Chair.

The Chairman announced that the President had appointed the following gentlemen Vice-Presidents: Col. Sabine, R.A., the Rev. Prof. Challis, Charles Giles Bridle Daubeny, M.D., the Very Rev. the Dean of Ely, Sir John Fred. Wm. Herschel, Bart., and William Spence, Esq.

The reading of Dr. Faraday's paper, entitled "Experimental Researches in Electricity. Twenty-eighth Series. On Lines of Magnetic Force; their definite character; and their distribution within a Magnet and through Space," which was commenced on the 27th November, was resumed and concluded.

The author defines a line of magnetic force to be that described by a very small magnetic needle, when it is so moved, in either direction correspondent to its length, as to remain constantly a tangent to the line of motion; or as that along which if a transverse wire be moved in either direction, there is no tendency to the formation of an electric current in the wire, whilst if moved in any other direction there is such a tendency. Such lines are indicated by iron filings sprinkled about a magnet. These lines have a determinate direction; they have opposite qualities in and about this direction, and the forces in any part of them are determinate for a given magnet. They may, as the author thinks, be employed with great advantage to represent the magnetic force as to its nature, condition, direction, and comparative amount; and that in many cases when other representations of the force, as centres of action, will not apply.

The term *line of force*, as defined above, is restricted to mean no more than the condition of the force in a given place, as to *strength* and *direction*; and not to include any idea of the nature of the physical cause of the phenomena: at the same time if reason should arise to think that the physical condition of the force partakes generally of the nature of a current or of a ray, a view which the author inclines to, he sees no objection in the term, any more than to the terms *current* and *ray*, as they are used in considerations regarding electricity and light, because it may accord with such a view.

The *lines of magnetic force*, as defined above, may be recognized either by a magnetic needle or by a moving wire; but the two methods are founded on very different conditions and actions of the magnetic force, and the moving wire appears to have the largest application. Its principle can be applied in places which are inaccessible to the needle, and it can sum up the forces in a given plane or surface at any distance from the central magnet. It has no reference to results of attraction or repulsion, and in some cases is opposed to them; but the author thinks it gives a true view of the disposition of the magnetic powers, and leads, and will lead to a more correct understanding of the nature of the force. For these reasons he advocates its adoption, not to the exclusion of the needle,

but in conjunction with it; and proceeds to develope the experimental methods and their results, and first in the case of a bar magnet.

Two bar magnets, each 12 inches long, 1 inch in width, and 0·4 of an inch in thickness, were fixed, side by side, a little apart, with like ends in the same direction, on and parallel to an axis, so that they might act as one bar magnet and be revolved at pleasure about the common axial line. A wire, which entering at one pole was carried along the axis of the magnetic arrangement, was at the centre turned outwards at the equatorial part, and then made to return at a distance outside the magnet to the place from whence it commenced. At times this wire was in three parts; the axial part being one; a radial part extending from the centre to the surface at the equator and there connected with a copper ring surrounding the magnet, being another; and the part from this ring on the outside of the magnet, back to the place of commencement, being the third; and each of these could revolve either separately or in conjunction with the other parts, the electric contact being complete in all the cases, whilst the wire was insulated from the magnet by the covering of silk. The ends of this loop, as it may be called, were connected with a galvanometer, and thus the presence or absence of electric currents ascertained, and their amount measured. Two galvanometers were used; one by Ruhmkorff, containing fine wire, and very delicate in its action; the other, constructed by the author, of copper wire 0·2 of an inch thick, passing only once round each needle; this, for abundant currents of low intensity, such as those generated in the moving wire, was found many-fold more delicate in its indications than the former.

The general relations of a moving wire to the magnetic lines of force are then specified, and a reference is made to their discovery and description by the author in the First Series of these Experimental Researches; and the law of the evolution of the induced electrical current is given. Referring to an easy natural standard, it may be said, that if a person in these latitudes, where the lines of force dip 69 degrees, as shown by the dipping-needle, move forward with arms extended, then the direction of an electric current which would tend to be produced in a wire represented by the arms, would be from the right hand through the arms and body to the left.

It will be seen, upon a little consideration, that a wire which touches a regular bar magnet at one end, and is then continued through the air until it touches it again at the equator, if moved once round the magnet, slipping at the equator contact so as to resume its first position at the end of the revolution, will have intersected, *once, all* the lines of force external to the magnet, and neither more nor less, whatever its course through the air, or distance in parts from the magnet, may be. Now when the external part of the loop above described is moved in this manner a certain number of degrees round the axis of the magnet, the latter being still, a current of electricity in a given direction is shown by the galvanometer; and the proper precautions (which are described)

being taken, the current is of the same amount for the same number of degrees of revolution, whether the motion be quicker or slower, or whether the wire be at a greater or a less distance in its course from the magnet.

If the external part of the loop be retained fixed, as also the axial part, and the magnet with the short radial part of the wire be revolved, an electric current is again produced, of a strength exactly equal to the former for the same number of degrees of revolution; but its *direction* is the reverse of the first current, when the direction of revolution is the same. In either case, reversing the direction of the revolution reverses the current produced by it. The moving radial part of the wire is in this case insulated from the magnet, and many other experiments, as with discs at the ends of the magnet, show, that the motion of the magnet itself is indifferent; and that whether it revolve or is still, provided the wire move, the result is the same. When the radial wire or part of the loop, and the external part move together, then their effects exactly neutralize each other, as they ought to do, being in contrary directions, for the same revolution; and not the slightest trace of a current under the extremest conditions of motion, or of the experiment, can be perceived. Such is the case, whatever the course or distance of the external part of the loop may be, or even when the loop is altogether external to the magnet, but moving at the same angular velocity either with or around it.

When the axial part of the loop is revolved it produces no effect; neither if this part revolve or be still does it produce the least influence on any of the results already described; it acts simply as a conductor, and is in other respects perfectly indifferent. This axial wire may be replaced by the magnet itself; for when it exists only from the magnetic pole outwards, and when the radial wire has contact with the magnets at the centre, so as to complete the electric circuit, the results are exactly the same as before: or the axial wire may proceed to the centre and then make contact with the magnet, and the radial wire be removed; when precisely the same results occur: or both axial and radial parts may be removed, the magnet serving both for conductor and moving radius, and still the results are unchanged.

From such results as these, the author draws the following conclusions, in relation to the *lines of magnetic force* as defined at the commencement. The amount of magnetic force (as shown by the electric current evolved) is determinate; and *the same* for the same lines of force, whatever the distance of the point or plane on which their power is exerted is from the magnet: or it is the same in any two or more sections of the same lines of force. There is no loss or destructibility, or evanescence or latent state of the magnetic power. Convergence or divergence of the lines of force causes no difference in the amount of their power. Obliquity of intersection causes no difference. In an equal field of magnetic force the electricity evolved is proportionate to the time of motion, or to the velocity of motion, or to the amount of lines of force intersected.

The *internal state* of the magnet is then examined by means of the results obtained with the radial wire, or the moving magnet when the latter makes part of the circuit; and the conclusion is arrived at, that there are within the magnet lines of magnetic force as defined as, and exactly equal in amount to, those outside of it; that these are continuations of the former; and that every line of magnetic force, whatever distance it may extend to from a magnet, (and in principle that is infinite,) is a closed curve, which in some part of its course passes through the magnet in conformity with what is called its polarity.

A current being thus induced in a closed wire, when it travels across magnetic lines of force, an inquiry is next made into the effect of altering the *mass* or *diameter* of the wire, and another form of apparatus is employed, in which loops of wire are made to intersect a given amount of lines; each loop consisting of a given length of wire, but either of wires of different diameter, or of one or more wires of the same diameter. The conclusion arrived at is, that the current or amount of electricity evolved is not simply as the space occupied by the *breadth* of the wire correspondent to the direction of the line of force, which has relation to the polarity of the power; nor by that *width* or dimension of it which includes the number or amount of lines of force intersected, and which, corresponding to the direction of the motion, has relation to the equatorial condition of the lines; but is jointly as the two, or as the mass of the wire.

The moving wire was next surrounded by different media, as air, alcohol, water, oil of turpentine, &c., but the result was the same in all.

Wires of different metals were used, and results in accordance with those obtained and described in the Second Series of these Researches were obtained: the conclusion is, that the current excited appears to be directly as the conducting power of the substance employed. It has no particular reference to the magnetic character of the body; for iron comes between tin and platinum, presenting no other distinction than that due to conducting power, and differing far less from these metals than they do from metals not magnetic.

Magnetic *polarity* then comes under consideration. The author understands by this phrase, the opposite and antithetical actions which are manifest at the opposite ends, or the opposite sides, of a limited portion of a line of force. He is of opinion that these qualities, or conditions, are not shown with certainty in every case, by attractions and repulsions; thus a solution of sulphate of iron will be attracted by a magnetic pole if surrounded by a solution weaker than itself, as shown in former researches on diamagnetic and paramagnetic action; but if surrounded by a solution stronger than itself it will be repelled. Yet the direction of the lines of force passing through it and the surrounding media cannot be reversed in these two cases, and therefore the polarity remains the same. The moving wire however shows, in similar cases, the true polarity or direction of the forces; and for an application of its principles, in this respect, to the metals, an apparatus is described by which discs of different metals can be revolved between the poles of a horse-shoe

magnet and the electric currents evolved in them carried off to the galvanometer. Now, whether the discs be of paramagnetic or diamagnetic metals, whether of iron, or bismuth, or copper, or tin, or lead, the direction of the current produced shows, that the lines of magnetic force passing through the metals is the *same* in all the cases, and hence the polarity within them the same.

The author then gives a more explicit meaning, in accordance with the definition of *line of magnetic force* contained in this paper, to some of the expressions used in the three last series of his Researches on Magnetic Condition, Atmospheric Magnetism, &c.: and by referring to former results obtained since the year 1830, illustrates how much the idea of lines of force has influenced the course of his investigations, and the results obtained at different times, and the extent to which he has been indebted to it; and then, recommending for many special reasons the mode of examining magnetic forces by the aid of a moving conductor, he brings for the present his subject to a conclusion.

December 18, 1851.

WILLIAM SPENCE, Esq., V.P., in the Chair.

The following papers were read:—

"A Proof (by means of a series) that every Number is composed of 4 Square Numbers, or less, without reference to the properties of Prime Numbers." By Sir Frederick Pollock, Lord Chief Baron, F.R.S. &c. Received December 18.

The paper contains a proof, that if every number of the form $8n+4$ is composed of 4 odd squares, then every number whatever must be composed of 4 square numbers or less; also a proof of the converse of this, viz. that if every number is composed of 4 square numbers or less, then every number of the form $8n+4$ must be composed of 4 odd squares.

It is then proposed to show that every number of the form $8n+4$ is composed of 4 odd squares, by taking a number of the form $8n+4$, viz. an odd square $+3$, and showing that $8n+4$ in that case is divisible into 4 odd squares (other than the odd square and 1, 1, 1); thus $16n^2 \pm 8n+1$ is a form that includes every odd square, and $16n^2 \pm 8n+4$ is divisible into

$$\begin{gathered} 4n^2 \pm 4n+1, \\ 4n^2 \pm 4n+1, \\ 4n^2 \pm 4n+1, \\ 4n^2 \mp 4n+1. \end{gathered}$$

8 is then added, and the sum is shown to be still divisible into 4 odd squares; and again 8, and so on, until by successive additions of $8+8+8$, &c., the quantity added to $16n^2 \pm 8n$ becomes equal to the original term with which the operation commenced. The odd squares $+3$ form the series 4, 12, 28, 52, 84, &c.; and if the suc-

cessive additions reach the third or some higher term, and also if the sum added to $16n^2+8n$ be equal to the term with which the operation commenced, it is contended the following term may be attained, and so on, and every number of the form $8n+4$ will be composed of 4 odd squares. The paper concludes by a suggestion that the method is applicable to several other similar problems.

2. "On the Valves of the Heart." By W. Savory, Esq. Communicated by Edward Stanley, Esq., F.R.S. Received September 30, 1852.

The paper contains observations upon the structure and connections of the auriculo-ventricular and arterial valves of the human heart, which the author thinks will assist in explaining their nature and functions.

The relation of the "four orifices" in the base of the heart is examined, and it is shown that the aortic and left auriculo-ventricular apertures are not separated as the others are; that no muscular tissue of the ventricle intervenes between them, but that when the auricles and great vessels are separated from the ventricles (which may be accomplished with facility after prolonged boiling), the aortic aperture is separated from the left auriculo-ventricular only by the anterior mitral valve; and when this is removed (or even while it remains), it is plainly seen that only one aperture exists whose borders are formed by the muscular tissue of the ventricle, and in shape somewhat resembling the figure 8. This is divided into two portions, an anterior (aortic) and posterior (auriculo-ventricular) by the anterior mitral valve, and above it, by the posterior wall of the aorta, into which is inserted a large portion of the anterior wall of the left auricle, but no muscular tissue of the ventricle intervenes.

When the auricles and vessels are removed, it is seen that the three orifices are bounded by thick and convex borders formed by the bases of the ventricles. Those on the left side are broadest; the difference between the two sides corresponding with the difference in thickness between the walls of the ventricles. The formation of these muscular borders, and the general arrangement and direction of the muscular fibres at these parts, is examined. The fibres forming the walls of the ventricles converge around these apertures. The most superficial fibres may be traced up from the walls of the ventricles, curving obliquely over the convex border, and having their extremities, for the most part, fixed around the orifices. We may remove these fibres layer after layer, and still find the same arrangement to obtain, the deeper fibres lying more transversely and obliquely intersecting those above and below.

If now the auricles and great vessels which have been detached are replaced in their natural situation, it is observed that the auricles are connected with the *inner* surface of these convex borders, while the walls of the vessels pass down on to the outer surface. This fact is an important one when viewed in connection with the valves, and will be presently considered. In the mean time it may be remarked, that the formation of the auriculo-ventricular grooves in which the coronary vessels lie, is explained. These vessels are

found in the angle between the border of the ventricles and the wall of the auricles.

The nature of the fibrous zones or tendinous circles surrounding the orifices is examined. These rings are in especial relation with the valves, being closely connected with their attached bases, and are not such distinct and independent structures as they have been hitherto considered. After referring to some previous descriptions of the arterial tendinous rings, the author attempts to show that what has been described as the upper and thickened festooned border, is the result of the attachment of the bases of the valves to the arterial coat, and is formed by an intimate union of the fibrous tissue composing the valves with the elastic coat of the artery.

(1) These festooned borders correspond exactly with the attached bases of the valves, and hence their shape. (2) They are thickest and most strongly marked at the angle formed by the junction of two valves, to which point the bands of fibrous tissue in the valves converge. (3) The microscope shows these festooned rings to be composed of a mixture of the white fibrous with the yellow elastic tissue, an arrangement naturally to be expected from an intimate union of the tendinous tissue of the valve with the arterial coat.

The structure, connections and relations of the valves is examined chiefly by means of vertical sections carried through their centres and adjacent parts. Such sections of the arterial valves disclose an important relation which they have with the upper border of the ventricles. The aorta and pulmonary artery, expanding towards their termination, are situated upon the outer edge of the ventricular border before described; the consequence of which arrangement is, that the portion of valve adjacent to the vessel passes over and rests upon the muscular substance, and is supported upon the inner border of the free edge of the ventricles surrounding the arterial orifices. This arrangement, in consequence of the small size of the parts, is not so obvious at the first glance in the human heart, but is more strikingly shown in an examination of the heart of any one of the larger animals. This appears of importance when viewed in connection with the functions of the valves. The reflux of the blood is said to be sustained by the festooned rings at the base of the valves, but in fact they are thinnest at this very part, corresponding to the central portion of the convexity of the valves; and if the description previously given of the formation of the tendinous festooned rings be a correct one, it is obvious why it is so, the thicker portions being the projecting angle at the junction of two valves, to which points the tendinous fibres of the valves converge. Now, inasmuch as the posterior portion of the aortic orifice is continuous with the left auriculo-ventricular aperture, no muscular tissue of the ventricle existing at this part, the posterior aortic valve, and a portion of the adjacent one, have no support of this kind; but the muscular floor of the anterior aortic valve is especially broad, and it is the corresponding portion of the aorta which is particularly dilated, the posterior wall descending nearly vertically. The arrangement above described obtains in all three pulmonary valves; but as the border as well as the

walls of the right ventricle are considerably thinner than those of the left, the muscular floor of these valves is much narrower than in the anterior aortic valve. All this is of course seen on a much larger scale in the hearts of the larger animals, as the Horse and Ox; and here, where the muscular floor of the valves (more especially the anterior aortic) is of very considerable breadth, the tendinous tissue of the valve may be traced over the muscular surface to form the wall of the vessel.

In the larger Ruminants there are found two considerable portions of bone, partly surrounding the orifice of the aorta, and smaller irregular fragments are occasionally observed between the principal pieces. The larger portions vary much in size and shape in different hearts even of the same species. They are usually elongated and curved. The chief bone, which exceeds the other considerably in size, embraces the whole of the right side, and the right half of the back part of the orifice of the aorta; while the little bone, not generally found in the smaller Ruminants, as the Sheep, its place being occupied by a portion of dense fibrous tissue, extends from the middle of the left side round to the posterior part, where it more or less nearly joins the extremity of the larger bone. Thus the lateral and posterior portions of the aortic orifice are surrounded by firm bony arches meeting posteriorly in the centre. From the large bone, a small process usually passes backwards for some distance into the muscular substance of the septum between the ventricles, and is gradually lost in the dense fibrous tissue found in this part, surrounding the right border of the left auriculo-ventricular aperture; and from the convex surface of the smaller portion, a thin process of dense fibrous tissue is continued round the left margins of the auriculo-ventricular orifice. These heart-bones are intimately connected above with the middle coat of the aorta, on the inner surface with the base of the adjacent arterial valves, and posteriorly with the anterior mitral valve; while at the sides, to their external and inferior surfaces, the muscular fibres of the ventricle are attached. They may be seen and felt in the base of the pouches formed by the two posterior aortic valves, and no doubt greatly assist in sustaining the "force of the reflux." They occupy the position of the two posterior festoons of the aortic valves. In the human heart, in the situation corresponding to the position of these heart-bones, the tissue composing the festooned rings is thicker and denser than elsewhere, offering to the knife, in some cases, almost the resistance of bone. The processes of dense fibrous tissue found in the anterior portion of the border of the ventricular septum, &c., and extending round the right and left margins of the auriculo-ventricular orifice, are intimately connected with the thickened portions of the adjacent festoons.

Among the tissues entering into the structure of the arterial valves, elastic fibres are described. They exist not only in the corpus arantii, but delicate fibres of elastic tissue are found throughout the valve; most abundantly in the thicker portions, but even in the thinner portions (lunulæ) a few delicate but well-marked elastic

fibres may be seen, particularly after the addition of acetic acid, which of course assists greatly in bringing them into view.

Muscular fibres have not been found in the arterial valves.

The structure and connections of the auriculo-ventricular valves are next examined by means of vertical sections. In tracing down the muscular wall of the auricle, it is observed to pass on to the inner surface of the ventricular border, and if minutely examined is seen to terminate by two attachments. The external portion, which is considerably the larger, is closely connected with the fibrous structure forming the "auriculo-ventricular ring," while the thinner internal portion is continued forwards for a very short distance between the surfaces of the valve, and terminates more or less abruptly by an attachment to its tendinous tissue. This is generally best seen in one of the tricuspid valves, where, in a vertical section, the muscular fibres may be observed terminating beneath its upper surface immediately beyond its attachment to the ring. In the posterior mitral valve the muscular fibres seldom penetrate so far forwards, and this appears to result, when a section of the parts is examined, from the much greater thickness and density of the lining membrane of the left auricle.

The connections of the anterior mitral valve, being peculiar, require a separate consideration. In dissecting down between the anterior wall of the left auricle and the posterior surface of the aorta, it is seen that the central fibres of the auricular wall are closely attached to the adjacent wall of the vessel. A little further dissection on either side will show that the muscular substance of the left ventricle is deficient between these parts. At the sides indeed it is found, but is gradually lost at some distance from the mesial line. Hence these two orifices (the aortic and left auriculo-ventricular) are not separated as the others are by the intervention of the muscular fibres of the ventricle. The structure and connections of the anterior mitral valve are examined by means of a vertical section, including the posterior wall of the aorta and the anterior wall of the left auricle. If the lining membrane of the auricle be traced downwards, it is found to be directly continued on to the posterior surface of the valve, and the membrane on the anterior surface of the valve is continued upwards over the tendinous festooned ring of the aorta, on to the under surface of its semilunar valves. The anterior mitral valve lies beneath a portion of the two posterior arterial valves. The muscular wall of the auricle is observed to terminate by two distinct insertions. The anterior (the larger) division of fibres is attached to the posterior surface of the aorta, opposite to, and below the festooned ring, while the posterior portion is continued directly downwards for a short distance into the valve, and terminates by an attachment to its fibrous tissue. The posterior wall of the aorta descends nearly vertically. Suddenly becoming much thinner opposite the upper border of the semilunar valve, it is continued down to the festooned ring, or in other words, it here becomes blended with the base of the semilunar valves. Below this a dense layer of fibrous tissue (which exists below, and fills up the spaces between

the attached bases of the semilunar valves) descends for some distance into the anterior mitral valve, immediately behind its anterior surface. It is by a close attachment to the posterior surface of this layer that the muscular fibres of the auricular wall which descend into the valve, terminate. This layer of fibrous tissue, however, may be generally traced downwards into the valve farther than the muscular fibres.

The boundary, then, between the aortic and auricular apertures is formed above the mitral valve by the posterior wall of the aorta, terminating at its junction with the bases of the semilunar valves, and immediately below the posterior surface of which is attached the greater portion of the muscular fibres forming the anterior wall of the left auricle. The extremities of the two bones which in ruminants replace a portion of the lateral and posterior divisions of the "festooned ring," nearly meeting in the centre, behind, give additional support to the structures entering into the formation of the mitral valve.

In examining the structure and connections of the auriculo-ventricular valves, it is noticed that a considerable portion of tendinous fibres pass from the insertions of the cords, through the valves, to the zones, and many of the smaller cords pass up directly into the angle formed between the under surface of the valve and the inner surface of the ventricle, and at once enter into the formations of the fibrous zones. These cords are short, and many of them spring from the wall of the ventricle, behind the valve. Therefore it results, that these zones are densest and most strongly marked in those portions corresponding to the attached borders of the valves, and gradually become less distinct towards the intervals between them. Hence the greater portion of the auriculo-ventricular zones is more properly to be considered in connection with the valves.

The fibres of elastic tissue exist in the auriculo-ventricular valves, but more sparingly than in the arterial valves.

The many contradictory statements which have been advanced concerning the existence of muscular fibres in the auriculo-ventricular valves, may perhaps be explained by a consideration of the mode in which the muscular fibres of the auricles terminate, which has been already described. The internal fibres which have been mentioned, descending from the auricular walls into the valves just beyond their attached margins, may be traced to a greater distance in some cases than in others. They generally terminate by a tolerably well-defined margin, but this varies. They usually descend for a greater distance between the layers of the anterior mitral valve, immediately beneath its auricular surface; but even here they are seldom found stretching far into the valve, not terminating, however, so abruptly.

Therefore, if a portion of the attached border of a valve immediately below its upper surface be examined, muscular fibres in abundance will generally be detected; whereas if sought for in any other portion of the valve far from its attached border, according to the foregoing observations, they will not be found.

January 8, 1852.

COLONEL SABINE, R.A., V.P. and Treas., in the Chair.

Charles Wheatstone Esq., F.R.S., delivered the Bakerian Lecture, being the substance of his paper entitled, "Contributions to the Physiology of Vision.—Part II. On some remarkable, and hitherto unobserved, phænomena of Binocular Vision,—(continued)." Received January 8.

The first part of these researches was communicated to the Royal Society in 1838, and published in the Philosophical Transactions for that year.

The second part, now presented, commences with an account of some remarkable illusions which occur when the usual relations which subsist between the magnitude of the pictures on the retinæ and the degree of inclination of the optic axes are disturbed. Under the ordinary circumstances of vision, when an object changes its distance from the observer, the magnitude of the pictures on the retinæ increases at the same time that the inclination of the optic axes becomes greater, and *vice versâ*, and the perceived magnitude of the object remains the same. The author wished to ascertain what would take place by causing the optic axes to assume every degree of convergence while the magnitude of the pictures on the retinæ remained the same; and, on the other hand, the phenomena which would be exhibited by maintaining the inclination of the optic axes constant while the magnitude of the pictures on the retinæ continually changed. To effect these purposes, he constructed a modification of his reflecting stereoscope; in this instrument two similar pictures are placed, on moveable arms, each opposite its respective mirror; these arms move round a common centre in such manner, that, however they are placed, the reflected images of each picture in the mirrors remains constantly at the same distance from the eye by which it is viewed; the pictures are also capable of sliding along these arms, so that they may be simultaneously brought nearer to, or removed further from, the mirrors. When the pictures remain at the same distance and the arms are removed round their centre, the reflected images, while their distances from the eyes remain unchanged, are displaced, so that a different inclination of the optic axes is required to cause them to coincide. When the arms remain in the same positions and the pictures are brought simultaneously nearer the mirrors, the reflected images are not displaced, and they always coincide with the same convergence of the optic axes; but the magnitude of the pictures on the retinæ becomes greater as the pictures approach. The experimental results afforded by this apparatus, so far as regards the perception of magnitude, are the following: the pictures being placed at such distances, and the arms moved to such positions, that the binocular image appears of its natural magnitude and its proper distance, on the arms being moved so as to occasion the optic axes to converge less, the image appears larger, and on their being moved so as to cause the optic axes to converge more,

the image appears less; thus, while the magnitude of the pictures on the retinæ remains constantly the same, the perceived magnitude of the object varies, through a very considerable range, with every degree of the convergence of the optic axes. The pictures and arms being again placed so that the magnitude and distance of the object appear the same as usual, and the arms being fixed so that the convergence of the optic axes does not change; while the pictures are brought nearer the mirrors the perceived magnitude of the object increases, and it decreases when they are removed further off; thus, while the inclination of the optic axes remains constant, the perceived magnitude of the object varies with every change in the magnitude of the pictures on the retinæ. After this the author takes into consideration the disturbances produced in our perception of distance under the same circumstances, and concludes that the facts thus experimentally ascertained regarding the perceptions of magnitude and distance, render necessary some modification in the prevalent theory regarding them.

The author next reverts to the stereoscope and its effects. He recommends the original reflecting stereoscope as the most efficient instrument, not only for investigating the phenomena of binocular vision, but also for exhibiting the greatest variety of stereoscopic effects, as it admits of every required adjustment, and pictures of any size may be placed in it. A very portable form of this instrument is then described, and also a refracting stereoscope suited for Daguerreotypes and small pictures not much exceeding the width between the eyes. In the latter instrument the pictures are placed side by side and viewed through two refracting prisms of small angle which displace the pictures laterally, that on the right side towards the left, and that on the left side towards the right, so that they appear to occupy the same place. When the first part of these investigations was published the photographic art was unknown, and the illustrations of the stereoscope were confined to outline and shaded perspective drawings; when, however, in the succeeding year, Talbot and Daguerre made their processes known, Mr. Wheatstone was enabled to obtain binocular Talbotypes and Daguerreotypes of statues, buildings, and even portraits of living persons, which, when presented in the stereoscope, no longer appeared as pictures, but as solid models of the objects from which they were taken. This application was first announced in 1841.

The two projections of an object, seen by the two eyes, are different according to the distance at which it is viewed; they become less dissimilar as that distance is greater, and, consequently, as the convergence of the optic axes becomes less. To a particular distance belongs a specific dissimilarity between the two pictures, and it is a point of interest to determine what would take place on viewing a pair of stereoscopic pictures with a different inclination of the optic axes than that for which they were intended. The result of this inquiry is, that if a pair of very dissimilar pictures is seen when the optic axes are nearly parallel, the distances between the near and remote points of the object appear exaggerated; and if, on the other

hand, a pair of pictures slightly dissimilar is seen when the optic axes converge very much, the appearance is that of a bas-relief. As no disagreeable or obviously incongruous effect is produced when two pictures, intended for a nearer convergence of the optic axes, are seen when the eyes are parallel or nearly so, we are able to avail ourselves of the means of augmenting the perceived magnitude of the binocular image mentioned at the commencement of this abstract. For this purpose the pictures, placed near the eyes, are caused to coincide when the optic axes are nearly parallel; and the diverging rays proceeding from the near pictures are rendered parallel by lenses of short focal distance placed before the mirrors or prisms of the stereoscope.

Some additional observations are next brought forward respecting those stereoscopic phenomena which the author, in his first memoir, called "conversions of relief." They may be produced in three different ways:—1st, by transposing the pictures from one eye to the other; 2ndly, by reflecting each picture separately, without transposition; and 3rdly, by inverting the pictures to each eye separately. The converse figure differs from the normal figure in this circumstance, that those points which appear most distant in the latter, are the nearest in the former, and *vice versâ*.

An account is then given of the construction and effects of an instrument for producing the conversion of the relief of any solid object to which it is directed. As this instrument conveys to the mind false perceptions of all external objects, the author calls it a Pseudoscope. It consists of two reflecting prisms, placed in a frame, with adjustments, so that, when applied to the eyes, each eye may separately see the reflected image of the projection which usually falls on that eye. This is not the case when the reflexion of an object is seen in a mirror; for then, not only are the projections separately reflected, but they are also transposed from one eye to the other, and therefore the conversion of relief does not take place. The pseudoscope being directed to an object, and adjusted so that the object shall appear of its proper size and at its usual distance, the distances of all other objects are inverted; all nearer objects appear more distant, and all more distant objects nearer. The conversion of relief of an object consists in the transposition of the distances of the points which compose it. With the pseudoscope we have a glance, as it were, into another visible world, in which external objects and our internal perceptions have no longer their habitual relations with each other. Among the remarkable illusions it occasions, the following are mentioned. The inside of a teacup appears a solid convex body; the effect is more striking if there are painted figures within the cup. A china vase, ornamented with coloured flowers in relief, appears to be a vertical section of the interior of the vase, with painted hollow impressions of the flowers. A small terrestrial globe appears a concave hemisphere; when the globe is turned on its axis, the appearance and disappearance of different portions of the map on its concave surface has a very singular effect. A bust regarded in front becomes a deep hollow

mask; when regarded in profile, the appearance is equally striking. A framed picture, hung against a wall, appears as if imbedded in a cavity made in the wall. An object placed before the wall of a room appears behind the wall, and as if an aperture of the proper dimensions had been made to allow it to be seen; if the object be illuminated by a candle, its shadow appears as far before the object as it actually is behind it.

The communication concludes with a variety of details relating to the conditions on which these phenomena depend, and with a description of some other methods of producing the pseudoscopic appearances.

January 15, 1852.

COLONEL SABINE, V.P., and Treas., in the Chair.

A paper was read, entitled, "On the Development of the Ductless Glands of the Chick." By Henry Gray, Demonstrator of Anatomy at St. George's Hospital. Communicated by W. Bowman, Esq., F.R.S. Received November 12, 1851.

In this paper the author has demonstrated the evolution of the spleen, supra-renal and thyroid glands, and the tissues of which each is composed, in order to show the place that may be assigned to each in a classification of the glands.

The *spleen* is shown to arise between the 4th and 5th days, in a fold of membrane which connects the intestinal canal to the spine (the "intestinal lamina"), as a small whitish mass of blastema, perfectly distinct from both the stomach and pancreas. This fold serves to retain it and the pancreas in connection with the intestine. This separation of the spleen from the pancreas is more distinct at an early period of its evolution than later, as the increased growth of both organs causes them to approximate more closely, but not more intimately with one another; hence probably the statement of Arnold, that the spleen *arises* from the pancreas. With the increase in the growth of the organ and the surrounding parts, it gradually attains the position that it occupies in the full-grown bird, in more immediate proximity with the stomach; hence probably the statement of Bischoff, that it *arises* from the stomach. Later, when its vessels are formed, the membrane in which it was developed is almost completely absorbed.

The author then considers the development of the tissues of the spleen, which clearly establishes, not only the glandular nature of the organ itself, but the great similarity it bears with the supra-renal and thyroid glands.

The external capsule and the trabecular tissue of the spleen are both developed between the 8th and 9th days, the former in the form of a thin membrane composed of nucleated fibres, the latter consisting of similar fibres, which intersect the organ at first sparingly, and afterwards in greater quantity. The development of the blood-

vessels and the blood are next examined. The former are shown to arise in the organ independent of those which are exterior to it. The development of the blood-globules is shown to arise from the blastema of the organ at the earliest period of its evolution, and continue their formation until its connection with the general vascular system is effected, at which period their development ceases. No destruction of the blood-globules could ever be observed. These observations disprove the two existing opinions of the use of the spleen, as the blood-discs are not formed there (excepting during its early development), as stated by Gerlach and Schäffner; nor are they destroyed there, as stated by Kölliker and Ecker.

The development of the pulp tissue is next examined. At an early period this closely corresponds with the structure of the supra-renal and thyroid glands at the earliest stages of their evolution, consisting of nuclei, nucleated vesicles, and a fine granular plasma, the former forming a very considerable portion of its structure. When the splenic vessels are formed, many of these nuclei are surrounded by a quantity of fine dark granules arranged in a circular form, and these increase up to the time when the splenic vein is formed, when nearly the whole mass is composed of nucleated vesicles, the nuclei of which gradually break up into a mass of granules which fill the cavities of the vesicles. The Malpighian vesicles are developed in the pulp by the aggregation of nuclei into circular masses, around which a fine membrane soon appears, in a manner precisely similar to those of the suprarenal and thyroid glands, with which they bear the closest analogy.

The author then traces out the development of the supra-renal glands, and shows the close analogy that exists between them, the spleen, and thyroid, from the similarity which their structure presents at the earliest period of their evolution with those glands, and from the development of the several tissues following the same stages in all.

They are shown to arise on the 7th day as two separate masses of blastema, situated between the upper end of the Woolfian bodies and the sides of the aorta, being totally independent (as concerns their development) of those bodies, or of each other. At this period their minute structure bears a close resemblance to that of the spleen, consisting of the same elements as that gland, excepting in the existence of more numerous dark granules, which give to the organ at a later period an opake and darkly granular texture. The gland tissue of the organ, in the form of large vesicles, makes its appearance on the 8th day, whereas in the spleen it did not exist until near to the close of incubation, an interesting fact in connection with the function of the former gland, which is mainly exercised during fœtal life, whilst the spleen exerts its function mainly in adult life; hence the difference in the development of the tissues at different periods. The manner in which this tissue is developed is similar to that by which the gland tissue of the spleen was formed, viz. by an aggregation of nuclei into circular masses, around which a limitary membrane ultimately forms: these

are first grouped together in a mass, without any subdivision into cortical and medullary portions. On the 14th day the first trace of this subdivision becomes manifest, by the vesicles being aggregated into masses which radiate from the circumference towards the centre of the gland, in some cases complete tubes being formed by the junction of the vesicles, as indicated by hemispherical bulgings along their walls. At a later period the organs increase in size, they attain their usual position, and a more complete subdivision into cortical and medullary portions is now observed.

The author lastly traces out the development of the thyroid glands, and shows the great similarity that exists between them, the spleen and supra-renal glands, from the similar structure they present, and from the development of those structures occurring in a similar manner in each.

These glands are developed between the 6th and 7th days as two separate masses of blastema, one at each side of the root of the neck, close to the separation of the carotid and subclavian vessels, and between the trachea and the branchial clefts, but quite independent, as far as regards their development, of either of those parts. Their minute structure at an early period closely corresponds with that of the spleen and supra-renal glands. Later, when the gland tissue of which the thyroid gland ultimately consists is formed, it is developed in a manner precisely similar to the same tissues of the spleen and supra-renal glands, a fact which shows the analogy they bear to one another.

From these observations the author concludes that a close analogy exists between the glands already described, so that the propriety of their classification under one group, as the "Ductless Glands," may be considered clearly proved. And although the spleen by many has been excluded from them, the author considers that its classification with them is correct, for the following reasons :—1st. From its evolution being similar with that of the supra-renal and thyroid gland; 2nd. from its structure, which at an early period closely corresponds with them; and 3rdly, from the development of its tissues following the same law as that upon which the tissues of the allied glands are formed.

January 22, 1852.

DR. DAUBENY, V.P., in the Chair.

A paper was read, entitled :—"Researches on the Geometrical Properties of Elliptic Integrals." By the Rev. James Booth, LL.D., F.R.S. &c. Received November 17, 1851.

In this paper the author proposes to investigate the true geometrical basis of that entire class of algebraical expressions, known to mathematicians as elliptic functions or integrals. He sets out by showing what had already been done in this department of the subject by preceding geometers. That the elliptic integral of the

second order represented an arc of a plane ellipse, was evident from the beginning. Hence indeed the name "elliptic functions," derived from a part, was given to the whole. Here then the question naturally arose: What geometrical types did the first and third orders represent? This question long remained without complete solution; and investigators in this department of analysis were compelled to take the fundamental expressions as arbitrary data, and to forgo the inquiry what the geometrical theorems were which these algebraical expressions represented. Various but unsuccessful attempts were made by geometers to represent them by quadratures, or by plane curves, either algebraical or transcendental. About ten years ago, however, Messrs. Guderman and Catalan showed that the circular form of the third order represented the curve of intersection of a cone of the second degree and a concentric sphere; but they did not extend their researches to the first order, nor to the logarithmic form of the third.

The main object of the paper is to prove that elliptic integrals of every kind, the parameter taking any value whatever between positive and negative infinity, represent the intersections of surfaces of the second order.

These surfaces divide themselves into two classes, of which the sphere and the paraboloid are the respective types; from the former arise the circular functions of the third order, from the other the logarithmic and exponential. In the course of these investigations it is shown that the formulæ for the comparison of elliptic integrals, which are given by Legendre, follow simply as geometrical inferences from the fundamental properties of those curves. The ordinary conic sections are merely particular cases of those more general curves, to which the author has given the name Hyperconic Sections.

The author remarks, that it will doubtless appear not a little singular, that the principal properties of these functions, their classification, their transformations, the comparison of elliptic integrals of the third order, with conjugate or reciprocal parameters, were all investigated and developed before geometers had any idea of the true geometrical origin of those functions. It is as if the formulæ of common trigonometry had been derived from an algebraical definition, before the geometrical conception of the circle had been admitted. As trigonometry may be defined, the development of the properties of circular arcs, whether described on a plane, or on the surface of a sphere, so this higher trigonometry, or the theory of elliptic integrals, may be defined as the development of the relations which exist between the arcs of hyperconic sections.

It may be said, we cannot by this method derive any properties of elliptic integrals which may not algebraically be deduced from the fundamental expressions appropriately assumed. It cannot, however, be truly asserted that the properties of curve lines should be developed without any reference to their geometrical types. We might, starting from certain algebraical expressions, derive every known property of curve lines, without having in any instance a conception of the geometrical types which they represent. The

theory of elliptic integrals was developed by a method the inverse of that pursued in establishing the formulæ of common trigonometry. In the latter case, the geometrical type was given—the circle—to determine the algebraical relations of its arcs. In the theory of elliptic integrals, the relations of the arcs of unknown curves are given, to determine the curves themselves; this is the principal object of the present communication.

The problem resolves itself into twelve distinct cases, depending on the magnitude of the parameter, and the sign with which it is affected; out of the discussion of these cases arise many new and important relations of elliptic integrals. It would excite little interest to give the bare enunciations of those theorems, and a mere outline of the methods by which they are established would be unintelligible. Not the least interesting of those theorems is the proposition, that it is always possible to express an elliptic integral of the first order as the sum of two elliptic integrals of the third order, with parameters which are conjugate, reciprocal and imaginary.

The author hopes, in a future communication to the Royal Society,—the present having grown under his hands beyond the limits he anticipated—among other points, to extend his researches to the case of elliptic integrals with imaginary parameters, and to show the true geometrical meaning of such expressions. It will also be shown, that imaginary expressions may be found for a logarithmic elliptic arc analogous to the well-known imaginary exponential expressions for the sines and cosines of circular arcs.

A paper was in part read, entitled, "Further Researches into the Structure, Development and Functions of the Liver." By C. Handfield Jones, M.D., F.R.S. Received November 19, 1851.

January 29, 1852.

COLONEL SABINE, R.A., Treasurer, in the Chair.

The reading of Dr. Handfield Jones' paper, "On the Structure of the Liver," was resumed and concluded.

Dr. Leidy and Professor Retzius, with Muller, Weber and Khronenberg, maintain the existence of plexuses of ducts in the parenchyma of the liver containing the cells in their tubes. Some other anatomists, especially Gerlach, believe the ducts to be prolonged into the lobules of the parenchyma, under the form of mere intercellular passages without walls.

Injections of acetate of lead in saturated solution, thrown into the ductus communis choledochus, produce appearances which seem to confirm the latter view. The author, however, believes them to be fallacious, and that the ducts really terminate, as he has described them in his former paper, by closed extremities, either rounded and even, or

somewhat irregular. Further details are given of the condition of the ultimate and penultimate ducts in the several vertebrate classes.

In the class of Fishes, the minute ducts most commonly appear as solid cylinders of soft granulous substance, in which scarce anything but some oily molecules are to be discerned; but not very unfrequently two other conditions are observed, which seem to illustrate very well the active character of the function of the duct. In the first the granulous matter exists in much smaller quantity, and the nuclei imbedded in it are consequently seen much more distinctly; their presence is thus unequivocally determined; it is shown that there is no real difference between the ducts of the fish's and those of the mammalian liver, only that the granulous matter is usually accumulated in the former more abundantly than in the latter. The presence of free nuclei in granulous matter indicates an active change to be proceeding in the part. In the second condition sometimes observed the granulous matter lies imbedded in it, a varying number of pellucid vesicles of great delicacy, but quite distinct; these testify that a process of active growth takes place in the minute ducts, and show, the author thinks conclusively, that the seminute ducts are not mere efferent canals.

Sugar was detected on two or three occasions in the livers of fishes; it seems to be absent when the organ is extremely fatty.

In the minute hepatic ducts of reptiles, the condition of the epithelium is very similar to that in fishes; the nuclei sometimes appearing with great distinctness, sometimes being obscured by much granulous matter, sometimes developing themselves into pellucid vesicles. The livers of frogs and toads almost constantly contain dark yellow masses which were formerly regarded by the author as biliary concretions, but are now considered to be only pigmentary deposits; they coexist sometimes with much diffused black matter.

The ultimate ducts have been traced recently very satisfactorily in Birds, Mammalia and Man, and the description given of them in the paper accords with the author's former account.

The development of the liver and its apparatus of ducts has been traced out in fishes and reptiles, and the following results obtained in both classes.

(1.) The liver (*i. e.* the parenchyma of the organ) is formed as an independent mass, and does not proceed as an effect from the intestine.

(2.) The gall-bladder is developed separately as a transparent vesicle, containing a clear fluid.

(3.) The gall-bladder elongates itself at one end, tends towards the intestine, and at last opens into it, while from one part of its extent hepatic ducts are developed; in the Frog the hepatic ducts seem, however, to be formed at the same time as the gall-bladder, and to be developed *pari passu* along with it. The cystic duct is lined by ciliary epithelium which plays very actively.

The examination of the process of development in the chick has confirmed, so far as it was carried, the account given in the former paper.

In Mammalia the subject of inquiry has been chiefly the following, viz. to ascertain how far there was evidence that the secretion of bile actually is effected in and by the hepatic cell, or whether its presence in them is accidental, and the bile is really and necessarily secreted by the ultimate ducts.

It is remarked that the existence of a portal vein conveying blood from the intestinal surface is coeval, not with the formation of a bile-secreting structure (for many animals have organs which secrete abundance of biliary-matter without any portal vein), but with the addition of a parenchymatous mass to the biliary organ, to which mass exclusively the portal vein is distributed. It is known that the parenchyma of the liver during, and for many hours after, digestion of food, forms, from the blood supplied to it, abundance of sugar, which thus appears to be its proper secretion; and it is not proved that the hepatic cells in a healthy state contain biliary matter, though they often do in various morbid conditions. Extracts of the hepatic parenchyma tested for bile by Pettenkoffer's method, give only very imperfect and doubtful traces of the presence of biliary matter, and on the other hand the sugar formed by the parenchyma, which is found so abundantly in the blood of the hepatic vein, is absent from the bile. The case of fatty liver, as occurring either pathologically or normally, seems also to require an explanation consonant with the view to which the above facts point, for otherwise it seems impossible to understand how perfectly formed dark-green bile could be contained in the efferent channels of a gland whose tissue is a mass of oil.

The structural condition of the ultimate biliary ducts is compared to that of the epithelium of the thyroidal cavities, and the nucleated granular tissue surrounding the lacteal in a villus; and it is shown to be probable that the terminal portions of the ducts,—so far as they possess the peculiar characteristic structure, exert an active elaborating energy, by means of which bile is formed or generated out of oily, albuminous or saccharine material which surrounds,—may be said to bathe them.

February 5, 1852.

SIR JOHN F. W. HERSCHEL, Bart., V.P. in the Chair.

The following papers were read:—

1. "Discovery that the veins of the Bat's wing, which are furnished with valves, are endowed with rythmical contractility, and that the onward flow of blood is accelerated at each contraction." By T. Wharton Jones, F.R.S., Fullerian Professor of Physiology in the Royal Institution of Great Britain, &c. Received November 20, 1851.

The author finds that the veins of the bat's wing contract and dilate rythmically, and that they are provided with valves; some of which completely oppose regurgitation of blood, others only par-

tially. The act of contraction of the vein is manifested by progressive constriction of its calibre and increasing thickness of its wall; the relaxation of the vessel, by a return to the former width of calibre and thickness of wall. The rythmical contractions and dilatations of the veins are continually going on, and that, on an average, at the rate of ten contractions in the minute. The contractions *centrad* and *distad* of a valve appear to be simultaneous, as also the dilatations.

During contraction, the flow of blood in the vein is accelerated, and on the cessation of the contraction, the flow is checked, with a tendency to regurgitation, which brings the valves into play. But this check to the onward flow of the blood is usually only momentary; already, even while the vein is in the act of again becoming dilated, the onward flow recommences and goes on, though with comparative slowness, until the vein contracts again. It is the heart's action which maintains the onward flow of blood during the dilatation of the vein, whilst it is the contraction of the vein, coming in aid of the heart's action, which causes the acceleration.

The valves are composed sometimes of but a single flap, sometimes of two. In the situation of a valve and *centrad* of the insertion of its flaps, the veins present the usual dilatations or sinuses. The valves are a reduplication of the clear innermost coat of the vein, with sometimes an intervening layer of cellular tissue.

The veins closely accompany the arteries, the nerve only intervening.

The contractility of the arteries the author finds altogether different from that of the veins, being *tonic*, not *rythmical*. He has not been able to observe unequivocal evidences of *tonic* contractility of the veins, which they have been alleged to possess.

In figure 3, of drawing No. 1, illustrating his paper, the author represents, in reference to this point, an artery and a vein, as observed immediately after pressure had been applied over them. The artery is seen constricted at intervals both above and below the place of pressure. The vein is not so constricted, but at the place where the pressure was applied there is seen a greyish granular deposit of lymph within the vessel, giving rise to an appearance of constriction by narrowing the stream of blood. On watching a vein in this state, the author has observed portions of the lymphy deposit carried away by the stream of blood, with corresponding enlargement of the channel.

The author further finds that nowhere do the arteries and veins of the web of the bat's wing directly communicate, as has also been alleged; the only communication being the usual one through the medium of capillaries.

In an appendix to this paper, the author describes the result of his microscopical examination of the structure of the veins and arteries. Both artery and vein have a middle coat of circularly disposed muscular fibres; but the appearance of the fibres is different in the two vessels. The fibres of the vein are $\frac{1}{3600}$dth in. broad, pale, grayish, semitransparent and granular looking. In general aspect,

they very much resemble the muscular fibres of the lymphatic hearts of the frog; but in none did the author detect an unequivocal appearance of transverse marking. The fibres of the middle coat of the artery are not so pale looking, are clearer, and exhibit a more strongly marked contour.

2. "Some Observations on the Ova of the Salmonidæ." By John Davy, M.D., F.R.S. Lond. and Ed., Inspector-General of Army Hospitals, &c. Received November 20, 1851.

The author prefaces his observations by a quotation from the work of M. Vogt on the Embryology of the Salmonidæ, in which a remarkable property of the vitellus is described, viz. its coagulation by admixture with water.

This inquirer's experiments were made chiefly on the ova of the *Palée* (*Coregonus Palæa*, Cuv.); the author's mostly on the ova of the Charr (*Salmo umbla*). After giving a description of the mature eggs of this fish, he details the trials instituted by him:—1st, on the action of water, showing its coagulating effect, except when added in very minute quantity. 2ndly, on the action of heat; how that a dry heat, even so high as that of 212° Fahr., occasions the contraction of the vitellus from evaporation, but not its coagulation, an effect even not produced by steam of the same temperature, but which is occasioned by boiling in water, owing, it is inferred, to an admixture of water. 3rdly, on the action of alkalies and salts; how these, such as potassa, ammonia and their sesquicarbonates in solution, nitre, acetate of lead, common salt and others, when of moderate strength, not only do not coagulate the vitellus, but have the property of dissolving a certain portion of coagulum, and coagulate it only when very much diluted. 4thly, on the action of acids and some other agents; how the vegetable acids tried, as the tartaric, oxalic, acetic, whether strong or dilute, do not coagulate the vitelline fluid, but dissolve its coagulum; how the strong sulphuric and muriatic acids inspissate it, the weak coagulating it; and further, how it is coagulated by the nitric acid, by corrosive sublimate and by alcohol, but not by iodine.

The inference from the experiments drawn by the author is, that the vitellus of the Charr and of the eggs of the other Salmonidæ is distinct in its properties, both from the albumen and yolk of the eggs of birds. He conjectures from analogy that the ova of other species of osseous fishes will be found to be similar; but not so those of the cartilaginous fishes. According to the observations he has made, the yolk of the eggs of fishes of this order, whether they possess a white, as in the instance of the oviparous; or are destitute of a white, as in that of the viviparous, resembles in its general character that of the egg of birds: but he doubts that the white of the former will be found analogous to that of the albumen ovi of birds, at least in its chemical qualities; having in one instance, that of the egg of the *Squalus Catulus*, found it to be, whilst transparent and viscid, neither coagulated by heat nor by nitric acid.

In conclusion, he suggests that the coagulation of the ova of the

Salmonidæ may have its use, inasmuch as the opaque white ova are more conspicuous than the transparent,—the dead than the living,—and in consequence, the one may serve as lures and divert from the others the many enemies to whom they are attractive food.

February 12, 1852.

COLONEL SABINE, R.A., V.P. and Treas., in the Chair.

The following communications were read:—

1. The subjoined Letter from Professor Haidinger to Captain Smyth, R.N., For. Sec. R.S., dated Vienna, January 15, 1852. Received January 23, 1852.

Sir,—The great success with which optical researches are treated of in the publications of the Royal Society, must make me anxious to lay before the Society, in a few words, a concise and convincing demonstration of the theorem that in a ray of polarized light the vibrations are perpendicular to the plane of polarization, conformably to the views of MM. Fresnel and Cauchy, and not in the plane of polarization, as some other mathematicians have maintained.

My demonstration is founded on the nature of dichroitic crystals, as tourmaline, sapphire, idocrase, &c. Any perfectly homogeneous crystal of this description presents two different tints of colours. One of them appears in the direction of the axis, as well as in all directions perpendicular to it, and it is always polarized in a plane passing through the axis; the other tint appears in every azimuth in the directions perpendicular to the axis, and it is polarized in a plane perpendicular to the axis. The latter of these colours does not appear at all, if the crystal is examined in the direction of the axis; if it depend at all on transverse vibrations, all vibrations of this kind, transverse or perpendicular to the axis, are at once excluded, and the only vibrations that can possibly belong to the colour of the extraordinary ray produced in the crystal, are those parallel to the direction of the axis. But agreeably to observation the plane of polarization is itself perpendicular to the axis, the *vibrations* therefore *take place* in directions *perpendicular to the plane of polarization.*

Trichroitic crystals of course will yield a similar demonstration, as cordierite, andalusite, diaspore, axinite, and others.

I shall not fail to send a copy of the communication I am to present today to the Vienna Academy, as soon as it shall have been printed.

The importance of the subject will, I am confident, plead as an apology for my trespassing on your kindness in thus making the request, that you will lay the present communication before the Royal Society.

I have the honour to be,
My dear Sir,
Your obedient Servant,
W. HAIDINGER.

2. A Letter to Sir John W. Lubbock, Bart., F.R.S. &c., "On the Stability of the Earth's Axis of Rotation." By Henry Hennessy, Esq., M.R.I.A. &c. Communicated by Sir John Lubbock. Received November 20, 1851.

The author refers to a communication to the Geological Society by Sir John Lubbock, in which he appeals, in support of the possibility of a change in the earth's axis, to the influence of two disturbing causes, which appear to have almost entirely escaped the notice of Laplace and Poisson in their investigations on the stability of the earth's axis of rotation:—1. The necessary displacement of the earth's interior strata arising from chemical and physical actions during the process of solidification. 2. The friction of the resisting medium in which the earth is supposed to move.

With reference to the first of these disturbing causes, the author states, that in his Researches in Terrestrial Physics (Philosophical Transactions, 1851, Part 2.), he has been led to conclusions which may assist in clearing up the question. From an inquiry into the process of the earth's solidification which appears to him most in accordance with mechanical and physical laws, he has deduced results respecting the earth's structure which throw some light on the changes which may take place in the relation between its principal moments of inertia, which relation is capable of being expressed by means of a function which depends on the arrangement of the earth's interior strata.

He then states that he has found strong confirmation of his peculiar views respecting the theory of the earth's figure, in the experiments of Professor Bischof of Bonn, on the contraction of granite and other rocks in passing from the fluid to the solid crystalline state. From the results of these experiments, he has been led to assign a new form to the function expressing the relation of the earth's principal moments of inertia. Referring to his paper for the mathematical processes by which he arrived at this result, he states that, from the theory he has ventured to adopt, it follows that, as solidification advances, the strata of equal pressure in the fluid spheroidal nucleus of the earth acquire increased ellipticity, and each stratum of equal density successively added to the inner surface of the solid crust is more oblate than the solid strata previously formed.

From these considerations alone, he remarks, it is evident that the difference between the greatest and least moment of inertia of the earth would progressively increase during the process of solidification. It follows, therefore, that if the earth's axis of rotation were at any time stable, it would continue so for ever. But from the laws of fluid equilibrium the axis must have been stable at the epoch of the first formation of the earth's crust; consequently it continued undisturbed as the thickness of the crust increased during the several geological formations. Thus it appears that the displacement of the earth's interior strata, instead of having a tendency to change its axis of rotation, tends to increase the stability of that axis.

With reference to inequalities arising from the friction of a resisting medium at the earth's surface, the author observes that they could not exist, if, as in the manner here shown, the axis of rotation coincided from the origin with the axis of figure.

In conclusion, he remarks, that if we could assume for the planets a similarity of physical constitution to that of the earth, the theorem as to the difference of the greatest and least moments of inertia of the earth would be applicable to all the planets; and thus we should be as well assured of the stability of our system, with respect to the motion of rotation of its several members, as we are already respecting their motion of translation.

In a postscript, referring to a third cause of disturbance in the place of the earth's axis of rotation, suggested in a letter from Sir John Lubbock, namely, the effects of local elevation and depressions at the earth's surface, the author states; if, with Humboldt, we regard the numbers expressing the mean heights of the several continents as indicators of the plutonic forces by which they have been upheaved, we shall readily see that these forces are of an inferior order to those affecting the general forms and structure of the earth. If the second class of forces acted so as not to influence in any way the stability of the earth's axis of rotation, the former class might, under certain conditions, produce a sensible change in the position of the axis. But when the tendency of the second class of forces is to increase the stability of the earth's axis, it would not be easy to show the possibility of such conditions as to render the operation of the other forces, not only effective in counteracting that tendency, but also capable of producing a sensible change in the place of the axis of rotation.

3. A paper was in part read, entitled, "On the Arrangement of the Foliation and Cleavage of the Rocks of the North of Scotland." By Daniel Sharpe, Esq., F.R.S., V.P.G.S. Received November 20, 1851.

February 19, 1852.

WILLIAM SPENCE, Esq., V.P., in the Chair.

The reading of Mr. Sharpe's paper, "On the Arrangement of the Foliation and Cleavage of the Rocks of the North of Scotland," was resumed and concluded.

The author applies the term, *cleavage* or *lamination*, to the divisional planes by which *stratified* rocks are split into parallel sheets, independently of the stratification; *foliation*, to the division of *crystalline* rocks into layers of different mineral substances; *slate*, to stratified rocks intersected by cleavage; and *schist*, to foliated rocks only which exhibit no bedding independent of the foliation.

He considers that no distinct line can be drawn between gneiss and mica schist, chlorite schist, &c., which pass from one into the

other by insensible gradations; have the same geological relations, and foliation subject to the same laws. He states that their boundaries have been laid down arbitrarily on the published maps of Scotland. The quartz rock of Macculloch includes two formations; the one, a quartzose variety of gneiss, included in this paper under that head; the other, a stratified sandstone altered by plutonic action.

The author treats the foliation of gneiss and schist as a series of simple curves, obtained by observing the general direction, and disregarding the minor and more complicated folds. The convolutions are usually greatest where the dip is slightest, but where the foliation is vertical or nearly so, it usually follows true planes without contortion; thus the most correct observations are those taken where the foliation is vertical.

When the foliation of gneiss and schist is traced over extensive areas, and the minor convolutions disregarded, it is usually found to form arches of great length and many miles in diameter, bounded by vertical planes, between which the inclination increases with the distance from the axis. Each arch is succeeded by a narrow space in which the dip is irregular, and beyond which another arch commences of a form similar to the first. Portions of two adjoining arches seen without the rest form the fan-like structure observed by several geologists. The arrangement of the foliation in arches corresponds with that of the cleavage of the true slates previously described by the author, except in the greater convolution of the gneiss and schist.

Along the southern border of the Highlands a band of stratified clay slate rests on mica schist: at the junction, the foliation of the schist conforms to the cleavage of the slate, and the two together form an arch, but there is no connection between the stratification of the slate and the foliation; moreover, the divisional planes cross from one rock to the other, without change of direction, being planes of foliation in the mica schist, and of cleavage in the slate: these facts confirm Mr. Darwin's opinion, that cleavage and foliation are due to the same cause.

The author describes the parallel arches of foliation which cross the Highlands, illustrating his description by sections and a map on which they are laid down, and tracing in detail the vertical planes which bound the arches. Commencing on the south, the first vertical plane runs about four miles within the Highland border, with a mean direction of about N. 55° E.: it crosses more than once the junction of the clay slate and mica schist. South of this plane the cleavage of the slate forms the beginning of an arch, which ends abruptly at the junction of the slate with the Old Red Sandstone.

To the north of this vertical plane four arches run across the Highlands: the most southern of these, with a diameter of ten or twelve miles, is formed partly of the cleavage of the slate, and partly of the foliation of the mica schist. The hills on the south side of Loch Tay coincide with its central axis. The vertical plane which forms its northern boundary crosses Ben Lawers, and has a mean direction

of N. 50° E. The next arch northward, consisting principally of gneiss, has a diameter varying from twenty-five to thirty miles; its axis runs for some distance along the central ridge of the Grampians. The granite of Cruachan and Ben Muich Dhui interfere with the regularity of the foliation of this district, and the lines are thrown to the north by the granite of Aberdeenshire: the line which bounds this arch on the north crosses the Spey near Laggan, and runs N. 40° E. through Corbine into the Monagh Leagh mountains. To the north of that line, the foliation of the gneiss forms an arch only ten miles wide, bounded on the north by a vertical plane running N. 35° E. which crosses Coryaraick. This plane forms the southern boundary of an arch, varying from fifteen to twenty-five miles wide, entirely of gneiss, bounded on the north by a band of vertical foliation which runs about N. 30° E. from Glen Finnan through the middle of Rosshire and across Ben Nevis. To the north-west of this band there is half an arch in the foliation, varying from twenty to thirty miles wide, which ends abruptly at a line to be drawn from Loch Eribol and Loch Maree, on the west of which the gneiss is unconformable to that hitherto described, but agrees with that of the Island of the Lewis, forming a series of arches which run about N.W.

From the want of parallelism in the lines of foliation of the Highlands, they would all nearly converge between Lough Foyle and Lough Swilly among the mica schists of the North of Ireland.

The most rugged and elevated hills are usually on or near the lines of vertical foliation; the axes of the arches are generally found in high land, and the principal valleys occur between the central axes of the arches and their vertical boundaries. Thus the main physical features of the Highlands are connected with the foliation of the gneiss and schists; but the granites and porphyries which have broken through those rocks, and disturbed the regularity of the foliation, have also greatly modified the surface of the country.

The contortions of gneiss and schists being unaccompanied by fracture, must, the author considers, have been produced when the matter of those rocks was semi-fluid: in this state the mineral ingredients appear to have separated and re-arranged themselves in layers according to their affinities, while the whole was subjected to pressure acting along certain axes of elevation, which raised those layers into arches.

February 26, 1852.

COLONEL SABINE, Treasurer and V.P., in the Chair.

The following paper was read:—

"On the Motions of the Iris." By B. E. Brodhurst, Esq., M.R.C.S. Communicated by Thomas Bell, Esq., Sec. R.S. Received November 20, 1851.

The observations made in this paper are distributed under three

heads. First, the author examines the iris in conjunction with the organic system of nerves. Secondly, he exposes the relation of the several nerves of the orbit in reference to the iris. And, thirdly, by tracing the membrane through the lower orders of animals, he shows the influence of the ophthalmic ganglion upon the iris, and the necessity of its presence for the accomplishment of the motions of the membrane, *i. e.* contraction and dilatation of the pupil.

It is shown that the pupil is most contracted during healthy sleep, and especially during that of childhood; that in death it assumes a median state, neither contracted nor dilated; and that, when disease is present, the pupil is always dilated, and dilated in accordance with the effect produced upon the trisplanchnic system of nerves. Also, it is stated that the pupil is dilated, when through disease the action of the voluntary muscles is abnormally increased, but that it is contracted when the functions of nutrition are well and actively performed; and that, with concussion and compression of the brain, the pupil is usually dilated when the power of the voluntary muscles yet remains; that it is fixed and immoveable when total insensibility exists; contracted when pressure or counter pressure is made upon the corpora quadrigemina of the opposite side, and dilated when the injury is more general, but less severe.

The author refers the first class of motions, or the *primary* motions of the iris, directly to the sympathetic system of nerves; whilst the *direct* movements, or those produced by the sensation of light, are effected through the cerebral nervous arc, as shown by Flourens, Marshall Hall, and others: and he thinks that contraction of the pupil, when a near object is presented to the eye, may be explained by the greater stimulus thus afforded to the retina and the sensorium; for he finds that when a near object is presented to the eye with a faint light, but a more distant one with a strong light, the pupil is most contracted for the more distant object. That the influence of the retina and the cerebral nervous arc is secondary in producing the motion of the iris, and that this membrane is not a mere diaphragm for the admission or exclusion of light, but that it yields to mental impressions, as well as to those which operate on the vegetative system of nerves, in preference to the effect upon the retina, is shown by the result which is produced upon the iris by any sudden passion in causing dilatation of the pupil, notwithstanding that a strong light be at the same time thrown upon the retina. Hippus, and the motions of the iris which are observed, especially in amaurotic children, are alluded to as motions independent of the light, and consequently, of the retina and sensorium.

The author then proceeds to state the effect of irritation and division of the several nerves of the orbit. He finds, that, on irritating the third nerve within the cranial cavity, slight contraction of the pupil ensues, to be followed by dilatation. On dividing the third nerve, the pupil becomes dilated beyond its median extent.

Irritation of the optic nerve within the cranium produces contraction of the pupil. Section of the same nerve gives rise to an insensible retina and a dilated pupil.

Irritation of the fifth nerve excites slight motion in the iris. Division of the fifth produces temporary contraction of the pupil. In the space of half an hour this effect will have ceased, and the pupil will have resumed its former diameter.

If a slight galvanic current be passed along the sympathetic, contraction of the pupil will be produced; but let the sympathetic be divided, at the superior cervical ganglion for instance, and instantly the pupil shall forcibly contract, and again widely dilate.

If a weak galvanic current be used, and the poles brought into contact with the sclerotica at its junction with the cornea, contraction of the pupil to two-thirds of its actual diameter takes place, and this effect continues so long as the current continues to be formed; but on breaking connection, the pupil immediately resumes its former diameter. So soon as life is extinct, galvanism ceases to affect the iris, whether applied to the membrane itself, through the external coats, or though the poles be in contact with the retina; but if applied to the sympathetic, movement may be excited in the iris.

The optic nerve being divided, the pupil is dilated: irritation of the third nerve then produces merely a slight and momentary effect upon the iris; but if the sympathetic be divided, the pupil will contract violently, and again dilate beyond its previous state of dilatation.

The sympathetic being divided, irritation of the cranial nerves does not affect the iris; but though the cerebrum and corpora quadrigemina be removed, division of the sympathetic will still excite the iris to motion. And, consequently, the author infers that the basic or primary motion of the iris is derived from the *vis motoria* of the excito-motor ganglionic system: he shows also, that where the ophthalmic ganglion is wanting, as in fishes and reptiles, the iris is motionless. Allusion is, lastly, made to some medicinal agents, to show their influence upon the nervous centres, and their consequent effect upon the iris: they are classed as follows:—

I. True depressors and pupil dilators.
II. True excitants and pupil dilators.
III. Stimulants which become depressors, which dilate the pupil.
IV. Excitors of voluntary nerves and pupil dilators.
V. Sedatives which terminate as depressors, which first contract and then dilate the pupil.
VI. Excitants which become sedatives, which first dilate and then contract the pupil.

And from what has gone before, it is concluded, that contraction of the pupil is the active state of the iris, and that dilatation is its enervated condition; that a healthy retina and cerebral nervous arc are necessary to the motions of the iris, and the ophthalmic ganglion to motion; and that the primary motion of the iris is due to organic nervous influence, but its forced or animal motion to the reflected stimulus of light upon the retina.

March 4, 1852.

WILLIAM SPENCE, Esq., V.P., in the Chair.

In accordance with the Statutes, the following List of Candidates for admission into the Society was read by the Secretary :—

George Appold, Esq.
Arthur Kett Barclay, Esq.
John Allan Broun, Esq.
Alexander Bryson, M.D.
Rev. Jonathan Cape, M.A.
Arthur Cayley, Esq.
Norman Chevers, M.D.
Hewitt Davis, Esq.
Robert Ellis, Esq.
John Hall Gladstone, Esq.
Henry Gray, Esq.
Rev. John Griffith.
Thomas Grissell, Esq.
Wyndham Harding, Esq.
John Hawkshaw, Esq.
Arthur Henfrey, Esq.
John Higginbottom, Esq.
Robert Hunt, Esq.
Edward Augustus Inglefield, Com. R.N.
Edward Joseph Lowe, Esq.
Robert Wilfred Skeffington Lutwidge, Esq.
John Mercer, Esq.
Hugh Lee Pattinson, Esq.
Rev. Bartholomew Price, M.A.
Lovell Augustus Reeve, Esq.
Julius Roberts, Lieut. R.M.A.
William Simms, Esq.
Hugh E. Strickland, Esq.
William Spottiswoode, Esq.
Robert Dundas Thomson, M.D.
John Tyndall, Esq.
Charles Vincent Walker, Esq.
Nathaniel Bagshaw Ward, Esq.
Charles Younghusband, Capt. R.A.

A paper was read, entitled, "On the Anatomy of *Doris*." By Albany Hancock, Esq., and Dennis Embleton, M.D., Lecturer on Anatomy and Physiology in the Newcastle-on-Tyne College of Medicine, in connection with the University of Durham. Communicated by Professor E. Forbes, F.R.S. Received November 27, 1851.

The authors have proposed to themselves to describe the anatomy of the three genera typical of the three groups of the Nudibranchiate Mollusca. An account of the structure of *Eolis* has already appeared in the 'Annals of Natural History.'

A detailed description is given of the anatomy of *Doris*, the following species of which have been examined, and are referred to in the paper: *D. tuberculata*, Auct., *D. tuberculata*, Verany, *D. Johnstoni*, *D. tomentosa*, *D. repanda*, *D. coccinea*, *D. verrucosa*, *D. pilosa*, *D. bilamellata*, *D. aspera*, and *D. depressa*; but *D. tuberculata* of English authors has been taken as the type of the genus, and the standard of comparison for the rest.

Digestive System.—The mouth in all the species is a powerful muscular organ, provided with a prehensile tongue beset with siliceous spines, which when the tongue is fully developed, are arranged in a median and two lateral series. Certain species possess, besides, a prehensile spinous collar on the buccal lip, occasionally associated with a rudimentary horny jaw. The mode of development of the lingual spines is shown to be the same as that of the teeth of the Vertebrata.

The œsophagus varies in length; in some it is dilated at the top,

forming a crop; in others it is simply enlarged previously to entering the liver mass. *The stomach* is of two forms; one, as in *D. tuberculata*, is very large, receiving the œsophagus behind, and giving off the intestine in front, and lying in advance of the liver; the other is received within the mass of the liver, and is very small. *The liver* in all is bulky, mostly bilobed, and variously coloured, and pours its secretion by one or more very wide ducts into the cardiac end of the stomach. A small laminated pouch—a rudimentary *pancreas*, is attached in some species to the cardiac, in others to the pyloric end of the stomach. The *intestine* is short, of nearly the same calibre throughout, rather sinuous in its course, and terminates in a nipple-formed anus in the centre of the branchial circle.

The Reproductive Organs are male, female and hermaphrodite. *The male organs* consist of penis and testis; the latter is connected with the former and with the oviduct. *The female organs* are, ovarium, oviduct, and mucus-gland. The ovarium is spread over the surface of the liver in the form of a branched duct with terminal ampullæ. The oviduct terminates in the mucus-gland. *The androgynous apparatus* is a tube or vagina opening from the exterior into the oviduct, having one or two diverticular spermathecæ communicating with it in its course. On the right margin of the body near the front is a common opening, to which converge the three parts of the reproductive organs. The spermatozoa are developed within large and fusiform spermatophora, and are observed in the spermathecæ, oviduct and ovary.

Organs of Circulation and Respiration.—The circulatory organs are, a systemic heart, arteries, lacunæ and veins. The existence of true capillaries in the liver-mass seems probable. A second heart—a ventricle, having a portal character, is also described. The systemic heart lies immediately beneath the dorsal skin, in front of the respiratory crown, and comprises an auricle and ventricle enclosed within a pericardium. In the systemic circle the blood is returned to the heart without having passed through the special respiratory organ. It is that blood only which is returned from the liver-mass that circulates through the branchiæ.

The authors conclude from their observations, that in the Mollusks there is a triple circulation: first, the systemic, in which the blood propelled along the arteries to the viscera and foot is returned, with the exception of that from the liver-mass, to the heart through the skin; there it becomes partially aërated, the skin being provided with vibratile cilia, and otherwise adapted as an instrument of respiration; second, the portal, in which venous blood from the system is driven by a special heart to the renal and hepatic organs, and probably to the ovarium, where it escapes, doubly venous, with the rest of the blood which has been supplied to these organs from the aorta, and which is therefore only singly venous, to the branchiæ; third, the branchial circulation, in which flows only the more deteriorated blood brought by the hepatic vein, but in which also that blood undergoes the highest degree of purification capable of being effected in the economy, namely in the special organ of respiration.

This triple circulation has not yet, as far as the authors are aware, been described as existing in the Molluscan Subkingdom. From the fact of the blood in *Doris* being returned to the heart in a state of partial aëration, it is clear, they say, that this animal is, in this respect, on a par with the higher crustaceans; and from the blood arriving at the heart in the same condition, according to the researches of Garner and Milne-Edwards, in *Ostrea* and *Pinna*, the great *Triton* of the Mediterranean, *Haliotis*, *Patella* and *Helix*, it can scarcely be doubted that this arrangement will be found throughout the Mollusca.

From a consideration of the facts cited in the paper, it may be deduced that the skin or mantle is in the Mollusca the fundamental organ of respiration, and that a portion of that envelope becomes evolved into a speciality as we trace upwards the development of the respiratory powers.

Upon the dorsal aspect of the liver-mass is a branched cavity, that of the *renal organ*, lined with a spongy tissue, and opening externally at the small orifice near the anus.

Organs of Innervation.—These are in two divisions, one corresponding to the cerebro-spinal division, the other to the sympathetic or ganglionic system of the Vertebrata. The existence of the latter, it is stated, is now for the first time fully established. The centres of the first system are seven pairs and a half of ganglia. Of the seven pairs, five are supra-œsophageal, two, infra-œsophageal: the single ganglion belongs to the right side and has been named *visceral*. There are three nervous collars around the œsophagus, one of which connects the infra- with the supra-œsophageal. The total number of pairs of nerves from the œsophageal centres is twenty-one, and there are also four single nerves.

The sympathetic system exists, and is more or less demonstrable, in the skin, the buccal mass, and on all the internal organs. It consists of a vast number of minute distinct ganglia, varying in size and form, the largest quite visible to the naked eye, of a bright orange colour, like the ganglia around the œsophagus, and interconnected by numerous delicate, white nervous filaments, arranged in more or less open plexuses. This beautiful system is connected with both sets of œsophageal ganglia.

The authors having found the sympathetic nervous system in several species of *Doris*, in *Eolis papillosa*, and in *Arion ater*, believe it to exist in all the more highly organized Mollusca.

The supra-œsophageal nervous centres in the Mollusca are in some instances so concentrated as to have led to the idea that they form only one mass; in others the ganglia are more or less distinct, and separated from each other. *Doris* has been taken as the representative of one class, *Aplysia* of the other, and on a comparison of both the supra- and infra-œsophageal ganglia of these with each other, there has been found a close correspondence between them, with the exception of the visceral ganglion. The single one in *Doris* is represented in *Aplysia* by a pair of ganglia, situated in the posterior part of the body near the root of the branchiæ. The supra-œsopha-

geal ganglia in the Lamellibranchiata appear homologous with those of *Doris*.

Having determined the existence of a true sympathetic or organic nervous system in *Doris*, the authors feel themselves more in a position to trace a parallelism between the œsophageal nervous centres of these Mollusca and the cerebro-spinal system of the Vertebrata, and accordingly they find there is a strict analogy between them, even to the individual pairs of ganglia of which they respectively consist, the general result being that the whole of the ganglia, grouped around the œsophagus in these Mollusca, answers to the encephalon, and a small portion of the enrachidion, of the Vertebrata.

Organs of the Senses.—The auditory capsules are microscopic, composed of two concentric vesicles, the inner enclosing numerous, oval, nucleated otolithes. *The eyes* are minute black dots, beneath the skin, attached by a pedicle to a small ganglion. They are made up of a cup of pigment, receiving from behind the nerve, and lodging in front a lens, having in advance of it a cornea, the whole enclosed by a fine capsule. The authors believe they have shown the dorsal tentacles to be the *olfactory organs*.

The organs of touch are, the general surface of the skin, but more particularly the oral tentacles or veil. *Taste* is most probably located in the lips and channel of the mouth, the tongue being a prehensile organ, and ill-adapted as the seat of such a function.

In conclusion, the authors comment on the high organization of the *Doridæ*, and express their belief that the genus, as at present understood, will require to be broken up into several groups.

March 11, 1852.

THE EARL OF ROSSE, President, in the Chair.

The following papers were read :—

1. "Remarks on certain points in Experiments on the Diffraction of Light." By the Rev. Baden Powell, M.A., F.R.S. &c., Savilian Professor of Geometry in the University of Oxford. Received Nov. 10, 1851.

The chief object of this communication was to examine into the experimental evidence adduced in a recent paper by Lord Brougham (Phil. Trans., 1850, Part I.), without at all entering on the question of the peculiar theory therein proposed, solely with the view of inquiring how far the actual new facts adduced, when simply stated and divested of the peculiar theoretical language in which they are delivered, do or do not militate against the undulatory theory.

The author had devoted a portion of the summer to a careful repetition of all the chief experiments described in the paper referred to, in some of which, however, he had been unable to reproduce the results described. After referring to the preliminary experiments

described in that paper, and the attempt there made to refute the theory of interferences, he remarks, that the theory of interferences explains perfectly *both* the internal and external fringes of a shadow; that the *breadth* of the fringes has no dependence on the *length of route* of the rays, but it has on the *angle* at which they intersect; and that interference also perfectly explains the fringes, even when the action is wholly *on one side* of the ray or edges.

Passing from these points of confessedly less importance, the author proceeds to consider the most material and fundamental experiment, in which, when fringes are formed by the edge of an opake body, if a second edge be placed at a greater distance along the ray, from the origin, on the *same* side as the first edge, it produces *no change* in the fringes; but on the opposite side it *does*, the fringes being shifted in position towards the first side; or, in other words, that the second edge, in the one case, has no power of producing further diffraction, in the other it has; and which has been viewed as supporting the theory of a peculiar action exerted by the edge upon the ray passing near it, by which it is disposed or indisposed for further *flexure* according to the conditions just expressed.

With reference to the edges on opposite sides, he observes, that when they are at the same distance from the origin, and form there a narrow aperture, they give (as is well known) fringes on each side extending into the shadow, with a white centre. As one edge is removed successively further from the origin along the ray and nearer to the screen, the fringes on that side dilate, become faint, and at length disappear; so that beyond a certain distance there remain only the fringes on the other side, or on that of the edge nearest the origin; which diverge further into the shadow on that side as the breadth of the effective aperture is diminished. In this way, then, the second edge, if beyond the limits of distance just mentioned, will cause an appearance of fringes on the side towards the first edge, diverging into the shadow.

When the two edges are at the same distance from the origin, forming a narrow aperture, the nature of the fringes is perfectly explained and reduced to quantitative results by Fresnel's theory. When the second edge is placed at a greater distance along the ray, this would be equivalent to a wide aperture placed obliquely to the direction of the ray, so as to be effectively as narrow as before. On the undulatory theory, this particular case has not been actually reduced to calculation, and it appears that it would certainly involve most complicated and difficult analysis to do so. It has however been treated in a general way by Fresnel himself (Mémoire sur la Diffraction, Mém. de l'Instit., tom. v. note, p. 452), who points out the general conditions for determining the condition of a fringe, and shows that the fringes will in this case undergo a modification, and will *not be symmetrical, but more expanded on one side than on the other*; which exactly agrees with observation.

After some remarks having the same bearing on other facts and propositions in the paper referred to, the author concludes by observing,—I have thus, I trust, with perfect impartiality, gone through

all the main experimental points of the investigation; and, upon the whole, I can perceive nothing substantiated which is positively *irreconcileable* with the principle of interference; while the new modifications of the phænomena here presented, so far as general considerations can be relied on, seem sufficiently conformable to the undulatory theory; but as to their more *exact* quantitative explanation, no definitive opinion can be pronounced until certain analytical investigations of great length and complexity shall have been gone through; by which alone theory can be brought into exact and satisfactory comparison with experiment.

2. "On the Lunar Atmospheric Tide at Singapore." By Captain C. M. Elliot, M.E., F.R.S. Received Dec. 18, 1851.

The discussion of the barometric observations at St. Helena by Colonel Sabine having clearly and decidedly shown the moon's influence on the atmosphere, the author determined to discuss in a similar manner the barometric observations at Singapore. The results of this discussion are given in the present communication.

In order that a comparison might be made between the results at Singapore and at St. Helena, he copied to a considerable extent the form of the different lunar tables drawn up by Colonel Sabine in his paper published in the Philosophical Transactions.

The observatory at Singapore was in latitude $1^\circ 18' 32''$ N. and longitude $103^\circ 56' 30''$ E. of Greenwich. The cistern of the barometer, one of Newman's, having a tube 0·532 inch in diameter, was a few feet above high-water mark. The observations, during the whole of 1841 and the early part of 1842 and that of 1843, were made at every two hours; during the remainder of the time, to the close of 1845, at every hour.

The diurnal variation of the barometer having been eliminated, by deducting the mean monthly height at each hour, from the height given by observation, the residual quantities were arranged in tables; and the observation corresponding the nearest in time to the moon's superior culmination for each day being marked as 0 hour of lunar time, the whole were again rearranged in tables according to lunar hours. The variation or range of the mean of the sums of the differences thus arranged is exhibited in a table, in the last column of which are given the means of all the hours for each period of six months. In a second table are given the differences between these mean results in the last column of the preceding table and the numbers corresponding to the several hours in the other columns.

The means of the complete years of observation, 1841, 1844, 1845, are shown in a third table, in which are also given the means of the first six months of 1842 and 1843, during which two-hourly observations were made, and the means of the latter halves of these years, during which the observations were made hourly.

The means of the twenty-four months of the two-hourly observations, and of the thirty-six months of the hourly observations, are given in Table IV. Finally, Table V. exhibits the results of the observations of three years, so combined as to show the effect on

the barometer, of the moon when similarly situated with reference both to its superior and inferior passage. In a column of this table are given the results of two years' observations at St. Helena, extracted from Colonel Sabine's paper. From a comparison, it appears that the effect produced by the moon on the barometer at Singapore, nearly on the equator, is slightly greater than at St. Helena, more distant from it by $14\frac{1}{2}^{\circ}$ of latitude.

March 18, 1852.

COLONEL SABINE, R.A., V.P. and Treas., in the Chair.

A paper was read, entitled, "On the Blood-proper and Chylo-aqueous Fluid of Invertebrate Animals." By Thomas Williams, M.D. Communicated by Thomas Bell, Esq., Sec. R.S. &c. Received Dec. 18, 1851.

In this paper the author has accumulated numerous observations, founded upon dissection and microscopic inquiry, to prove that there exist in *invertebrate animals* two distinct kinds of nutrient fluids; that in some classes of this sub-kingdom these two fluids coexist in the same organism, though contained in distinct systems of conduits, while in others they become united into one. The author proposes to distinguish these two orders of fluids under the denominations of the *blood-proper* and *chylo-aqueous fluid*. The former is always contained in definitively organized (walled) blood-vessels, and having a determinate circulatory movement; the latter, with equal constancy, in chambers and irregular cavities and cells, communicating invariably with the peritoneal space, having not a determinate circulation, but a to-and-fro movement, maintained by muscular and ciliary agency. He then adduces evidence, derived from dissection, in proof of the statement that the system of the blood-proper does not exist under any form, the most rudimentary, below the Echinodermata; that, in other words, the system of the true blood, or of the blood-proper, begins at the Echinodermata. The author then shows that below the Echinodermata, namely in the families of Polypes and Acalephæ, the digestive and circulatory systems are identified, and that consequently the external medium is admitted directly into the nutrient fluids. He considers that this circumstance constitutes a fundamental distinction between the chylo-aqueous system and that of the blood-proper, into which, under no conditions, is the external inorganic element directly introduced.

He conceives that his observations suffice to establish the law, with reference to the chylo-aqueous fluid, that in every class in which it exists, it is charged more or less abundantly with *organized corpuscles*. This is an invariable fact in the history of this fluid. His inquiries show that these corpuscles are marked by distinctive microscopic characters, not in different classes and genera only, but

in different species, entitling these bodies to great consideration in the establishment of species.

The paper then proceeds to demonstrate the proposition, that in those classes, as in the Echinodermata, Entozoa and Annelida, in which, in the adult animal, these two orders of fluids coexist, though distinct, in the same individual, there prevails between them, as respects their magnitude or development, an inverse proportion; that while, as instanced in the Echinoderms, the chylo-aqueous fluid filling the ciliated space between the stomach and integument is *considerable* in volume, the blood-proper and *its* system are *little* evolved; that while, as in the Entozoa, the chylo-aqueous fluid is still the most important fluid element in the organism, the blood system is proportionally rudimentary; that in the Annelida, especially the higher species of that class, the chylo-aqueous fluid almost disappears, while the system of the true blood acquires, illustrating the law of inverse proportion, a correspondingly-augmented development. The author then states, that the system of the chylo-aqueous fluid does not exist in the *adult*, but only in the *larva* state of the higher members of the articulated series, such as the Myriapoda, Insecta and Crustacea.

In Myriapods and Insects, he has observed that the peritoneal space is occupied by a fluid which does not communicate with, and is distinct in composition from, the contents of the true blood-vessels.

This peritoneal fluid, however, in these classes disappears at a subsequent stage of growth. Thus the author thinks that a continuous chain, *through the medium of the fluids*, is established between the Echinoderms at one extreme and the Crustacea at the other. These classes he proposes to connect together under the designation of the *double fluid series*, corresponding to the radiate and articulate series of systematic zoologists.

Returning to the standard of the Echinoderms, where the system of the blood-proper first appears in the zoological scale, he shows that at this point the *Molluscan* chain diverges from the radiate and articulate chain, and may be indicated, in contradistinction from the latter, as the *single-fluid series*. The author's observations lead him to believe, with Professor Milne-Edwards, that in all Molluscs, from the Tunicata to the Cephalopods, the chamber of the peritoneal is continuous with the channels of the circulation, and that consequently the fluids observed in these parts are *one and the same fluid*, establishing the *singleness* of the fluid system of the body; and this conclusion is corroborated by additional evidence drawn from microscopic examinations.

He then recapitulates the results of his researches, and maintains that the base of the invertebrated kingdom of animals is formed of all those inferior series which rank below the Echinoderms; and that this series is distinguished from the Molluscan, in which also the fluid system is single, by the important circumstance that in the former, unlike the Mollusca, the digestive and circulatory system are identified, or confounded into a single system; that at the Echino-

derms the series divaricates into the double-fluid series and single-fluid series, the former coinciding with the radiate and articulate class, and joining the Vertebrata through the Crustacea; the latter running parallel with the Molluscan order, and connecting itself to the Vertebrata through the Cephalopods.

The fluids of the zoophytic series are invariably corpusculated, but the corpuscles cannot yet be reduced to any definite type of conformation. In the Medusan series these bodies become more definitively organized. The author then demonstrates, that throughout the whole radiate and articulate classes, wherever it is found, the *chylo-aqueous* fluid is richly corpusculated, or in other words, charged with floating morphotic elements, which, from the constancy of their characters in different species, become grounds for specific distinctions. It is stated, that, throughout the Echinoderms, Entozoa and Annelida, in which, even in the adult animal, the blood-proper and the chylo-aqueous fluid, though separate, coexist, the latter fluid only is corpusculated, the true blood being invariably limpid and perfectly fluid (incorpusculated), and almost always the seat of the colour; the latter existing as a substance dissolved in the fluid, while in no instance does colour develope itself in the chylo-aqueous fluid.

The paper then shows, that at the point where the chylo-aqueous system disappears, namely at the Myriapods, the true blood becomes the vehicle of the corpuscles.

And lastly, the author adduces a great variety of observations in confirmation of the statement, that throughout the whole Molluscan series without exception, coinciding with his "*single-fluid series*," the fluids are richly charged with corpuscles.

The paper is accompanied by numerous illustrations, displaying the characters of the morphotic elements of the circulating fluids of the Invertebrata.

March 25, 1852.

SIR J. HERSCHEL, Bart., V.P., in the Chair.

A paper was in part read, entitled, "Experimental Researches in Electricity: Twenty-ninth Series." By Michael Faraday, Esq., D.C.L., F.R.S. &c. Received Dec. 31, 1851.

April 1, 1852.

COLONEL SABINE, R.A., V.P. and Treas., in the Chair.

The reading of Dr. Faraday's paper, "Experimental Researches in Electricity: Twenty-ninth Series," was resumed and concluded.

In the present series of researches the author endeavours in the first place to establish the principles he announced in the last, with regard to the definite character of the lines of magnetic force, by

results obtained experimentally with the magnetic force of the earth. For this purpose he reverts to the thick wire galvanometer before described, and points out the precautions respecting the cleanliness of the coils, the thickness and shortness of the conductors, the perfect contacts, effected either by soldering or cups of mercury; and marks the value of double observations, i. e. observations afforded on both sides of zero. The nature of the impulse on the needles is pointed out; being not that of a constant current for a limited or unlimited time, but of a given amount of electricity exerted, either regularly or irregularly, within a short period; and it is shown experimentally that such impulses produce equal results of deflection, and also that when two or more such impulses are given within a limited time, the whole arc of swing is nearly proportional to their number; so that the amount of deflection, *within certain limits*, indicates directly, nearly the proportion of electricity which has passed as a current through the instrument.

If a wire be formed into a square of 12 inches in the side, and then fixed on an axis passing across the middle parallel to two of its sides, and if, when that axis is perpendicular to the line of dip, the whole is rotated, then two of the sides of the rectangle will, in one revolution, twice intersect the lines of force of the earth passing across or through one square foot of area. The currents then tending to move in the upper and lower parts of the rectangle, will conjoin to urge one current through the wire; and if this wire be cut at one place close to the axis, and be there connected with a commutator of simple construction, which is described in the paper, the currents round the rectangle may be conveyed away to the galvanometer, and there measured. Such a rectangle, constructed of copper wire one-twentieth of an inch in thickness, gave a certain arc of swing for one revolution. If five or ten revolutions were made, within the time of vibrating of the needle, nearly five or ten times this amount of deflection was produced: the mean result, in the present case, was $2^\circ{\cdot}624$ per revolution. When the same length of the same wire was arranged in oblong or oblate rectangles, so as to diminish the inclosed area in different directions as regarded the axis of revolution, still the deflection was in every case proportional to the areas included; showing that the effect produced was proportional to the number of lines of force intersected by the moving wire. The same result was obtained when two squares having areas in the proportion of 1 to 9, were employed.

When squares of the same area were formed of copper wire of different thicknesses, then the effects of obstruction in the conducting part of the system were brought out and measured. Thus, with wires which were 0·05, 0·1 and 0·2 of an inch in diameter, and therefore in mass as 1, 4, and 16, the deflections were 1, 2·78, and 3·45; a result almost identical with that obtained for the same wires by the use of loops and a local magnet in the former researches. When two equal rectangles were compared, one containing a single circuit of 4 feet of wire 0·1 in thickness, and the other four circuits of 16 feet of wire 0·05 in thickness, then the first was found to

evolve the largest quantity of electricity; but the second, electricity of the highest tension, by the same amount of motion: the accordance of these results with the principles advanced is pointed out. The author then refers to the use of wire rings of one or many convolutions, and indicates cases in which they may supply valuable means of experimental inquiry.

The relative amount and disposition of the forces of a magnet when it is alone, or associated with other magnets, forms the next point in the present paper; and a distinction is first taken between ordinary magnets, which are influenced much by other magnets, so that the amount of their external force varies greatly, and those which are very hard, where this influence is reduced to little or nothing. The power of a given magnet was measured according to the method described in the last series, by a loop once passed over its pole. A given hard magnet placed in an invariable position, being thus estimated, was found to have a force equivalent to $16^\circ{\cdot}3$ of deflection. Another magnet, having a power of $25^\circ{\cdot}74$, was then placed close to the first in different positions, with like or unlike poles near together, so as to tend sometimes to exalt its power and at other times to depress it; and the results observed. In the extremest favourable case, namely, when the two were conjoined as a horseshoe magnet, the force of the first magnet was only raised $2^\circ{\cdot}45$, which fell directly the dominant magnet was removed; in the corresponding adverse case the depression was only 1°. A very hard magnet, made by Dr. Scoresby, of $6^\circ{\cdot}88$ power, when under the influence of another of double its power, was not sensibly affected either way. When under the influence of one of six times its force, it could be affected to the extent of nearly 1°. Ordinary magnets can be affected to the extent of one half of their power or more; and indeed in extreme cases can be altogether overruled and inverted.

From these results the author concludes, that, with perfect unchangeable magnets, the lines of force (as before defined) of different magnets in favourable positions, coalesce: that there is no increase of the total force by this coalescence; the sections between the associated poles giving the same sum of power as the sections of the lines of either magnet alone: that as the external amount of force of the magnet is not varied, neither is the internal amount at all changed: that the increase of power upon a magnetic needle, or a piece of soft iron, placed between two opposite favourable poles, is caused by concentration of the lines which before were diffused, and not by the addition of the power represented by the lines of force of one pole to that of the lines of force of the other. There is no more power represented by *all* the lines of force than before, and a line of force is not in itself more powerful because it coalesces with a line of force of another magnet. In this and in other respects, the analogy of the magnet with the voltaic pile is perfect.

The paper concludes with some practical remarks upon the delineation of the forms of the lines of force by iron filings, and by a description of the inflection of the lines by hemispheres of hot and cold nickel; which the author considers as the corresponding case to the action of warm and cold oxygen in the atmosphere, as applied

by him in the explication of some of the phenomena of atmospheric magnetism, and especially of the annual and daily variation.

A paper was also read, entitled, "On the Electro-chemical Polarity of Gases." By W. R. Grove, Esq., M.A., F.R.S., &c. Received January 7, 1852.

The author refers to the experiments of Faraday on dielectric induction, to those of Gassiot on the increase of electrical effects of tension, according as the chemical intensities of a voltaic battery are increased, and to other results, which, though supporting the view of a physico-polar state of gaseous substances intervening between oppositely electrified surfaces, have not hitherto shown any change in the arrangement of the gaseous particles dependent upon their chemical characteristics.

The electric or voltaic disruptive discharge has hitherto presented only one phenomenon which offers any analogy to electrolysis, viz. that observed by Mr. Gassiot and others, of the positive terminal being more intensely heated than the negative, when the voltaic discharge passes between metals. With the voltaic arc the effects of heat and the destruction of the terminals so interfere with any effects properly due to the transmission of the electric current, that it is next to impossible to eliminate the latter; on the other hand, with the electric spark from an ordinary machine, the quantity of matter acted on is too minute to give satisfactory evidence of the changes taking place. Mr. Grove sought an intermediate degree of electrical action, and by the aid of an apparatus of Ruhmkorf for producing a powerful secondary current, the results detailed in this paper were mainly obtained.

A polished silver plate is laid on the pump plate of a good air-pump, and a metallic point is attached to the rod passing through a collar of leathers at the top of the receiver, the point being adjusted at from one-eighth to one-fourth of an inch distance from the plate. Caustic potash is kept suspended in the receiver, and a mixture of oxygen and hydrogen, or atmospheric air and hydrogen, allowed to enter it, and then attenuated until the barometer stands at half an inch; the discharges from the secondary coil are now made to pass between the point and the plate, when if the latter be positive it is oxidated, if negative the spot of oxide is reduced.

If there be excess of oxygen and little or no hydrogen, oxidation takes place, whether the plate be positive or negative, though in different degrees; and if the gas be wholly or mainly hydrogen, reduction takes place whether the plate be positive or negative.

At certain intermediate states of mixture rings or zones of alternate oxidation and reduction are shown, quite distinguishable from the ordinary succession of colours of thin plates, and showing alternations or periods of interference of electrical action.

The author then gives the results of experiments with several other metals, of which bismuth was the only one he found to produce effects anything like equal to the silver, though other metals showed them in some degree.

He also varied the gas or gases employed, and details the results

obtained with several gases; among them carbonic oxide is the most worthy of note, as with it effects are produced similar to those with the mixture of oxygen and hydrogen, viz. oxidation when the plate was positive, and reduction when it was negative.

The author's theory or mode of explaining the results is as follows. The discharges are successive, not continuous, and antecedent to each discharge the intervening gas is thrown into a state of chemical polarity, similar to that which takes place in an electrolyte anterior to electrolysis; by this means the positive terminal has in juxtaposition with it oxygen or an electro-negative gas; the discharge takes place, and by the superficial ignition the layer of oxygen combines with the metal in contact with it.

Conversely, when the oxidated surface is negative and in contact with an electro-positive gas, the heat of the discharge produces reduction. The fact of oxidation only taking place when air or oxygen alone are present, and reduction only when hydrogen is present, he considers irreconcilable with the effects being attributable to the discharge itself, or to their being regarded as analogous to electrolysis; while these phenomena are corroborative of the view he puts forth.

The author refers to the experiments of Priestley, Karsten and others, in which spots or marks have been shown to be produced by electrical discharge, but which do not otherwise bear upon the objects sought to be elucidated by this paper.

The Society then adjourned to April the 22nd.

April 22, 1852.

The EARL OF ROSSE, President, in the Chair.

G. E. Day, M.D., was admitted.

The following papers were read:—

1. "On the Structure of the Stem of *Victoria regia*." By Arthur Henfrey, F.L.S. &c. Communicated by Professor Edward Forbes, F.R.S. Received February 19, 1852.

The investigation of the anatomy of *Victoria regia* acquires its interest from the fact of the relations which have been pointed out to exist between the Nymphæaceæ and some of the undoubted Monocotyledonous families, especially also from the researches of M. Trécul on the anatomy of *Nuphar lutea*, which plant that author describes as having a stem of the Monocotyledonous type of structure. Through the unfortunate death of the plant of *Victoria regia*, which had flowered for some time in the gardens of the Royal Botanic Society of London, the author had an opportunity of examining the anatomy of its stem. It is an upright rhizome, with undeveloped internodes, growing by a single terminal bud, apparently perennially, and attaining considerable thickness; on the outside it bears the remains of the petioles and flower-stalks, which separate by disarticulation, and their remains are found arranged in

spiral lines upon the outside, so as to give the short, thick rhizome the aspect of a piece of a palm stem. As in *Nuphar*, the roots are produced in bundles at the bases of the petioles, and fall off successively upwards as the new ones are developed, leaving very conspicuous scars. The internal structure of the stem is quite Monocotyledonous in its character, presenting no trace of the arrangement of the vascular bundles into rings of wood, no true woody fibres, and no cambium layer. The vascular bundles, which are composed exclusively of spiral, annular and reticulated ducts surrounded by elongated parenchymatous cellular tissue, are isolated and arranged just as in Monocotyledons, such as the Palms; and the outer part of the stem exhibits a cortical parenchyma, much more like that of the herbaceous rhizomes of the rush-like plants, than any other known structure; it bears not the least resemblance to the bark of Dicotyledons. The results of the investigation show that *Victoria*, like *Nuphar*, has a stem of essentially Monocotyledonous structure. The paper was accompanied by drawings illustrating the general and microscopic anatomy of the stem.

2. "On the Meteorology of the English Lake District, including the results of Observations on the Fall of Rain at various heights, up to 3166 feet above the Sea-Level:" Fifth paper, for the year 1851. By John Fletcher Miller, Esq., F.R.S. &c. Received March 1, 1852.

The author states that the results for the past year do not seem to call for any particular remarks, and as it appears desirable, as a general rule, to defer all attempts at deduction until after the completion of the observations, the Tables for 1851 are presented, without many notes or comments, in continuation of the series which have previously appeared in the Transactions of the Society. The table for January, 1851, is given as an example of the daily fall of rain in the district during an excessively wet month, and also as showing the form of permanently registering the returns from the various stations, when sent in at the close of each month. He remarks that the quantity of 38·86 inches precipitated on "The Stye" in January 1851, is, he believes, without a parallel in the temperate zone.

3. "Formulization of Horary Observations presumed *à priori* to be nearly of a Periodic nature." By S. M. Drach, Esq., F.R.A.S., F.R.G.S. Communicated by Colonel Sabine, R.A., Treas., V.P.R.S. &c. Received March 18, 1852.

Referring to his former publications on the subject (Proceed. Roy. Soc. March 1842, Phil. Mag. 1842–51), the author empirically resolves the formula

$$ht = \mathrm{H} + \Sigma \mathrm{A}_i \sin it + \Sigma a_i \cos it = \mathrm{H} + \Sigma \mathrm{R}_i \sin (it + \psi_i),$$

h being the effect observed at the hour-angle t, thus obtaining from the 24 hourly observations all values up to $i=12$. This method giving the values of A_i, a_i, R for the different months, he believes that by it the law of change connected with the sun's motion in

longitude and declination will be most readily deduced. The formula is exemplified by calculations and results of the diurnal variation of magnetic declination for each month at the various Colonial Observatories, and also of the temperature at the Cape, St. Helena, Hobarton, Toronto, Greenwich, Leith, and Melville Island. The author infers *that the temperatures taken at six-hourly intervals give for their sum four times the mean temperature of the day, whatever be the commencing hour*; and thus travellers and voyagers observing at 5^h, 11^h, 17^h and 23^h, will get the mean temperature of their position at 2 P.M. Hence, from the communications of the captains of Merchantmen, the Atlantic oceanic temperatures might be mapped in the course of a year, and the isothermal curves on this broad level surface be accurately laid down (see Journ. R. Geograph. Soc. ix. p. 369). Excepting at Melville Island, R_1 is the greatest coefficient, ψ_1 is nearly constant, and

$$H+\Sigma_1^4 R_i \sin(it+\psi_i)+\cos 8t \cos 2t(F\sin t+G\cos t)$$

will give the *yearly* formula: the homonymous hours are expressed by $H+\Sigma_1^2 R_i \sin(it+\psi_i)$ as in the oceanic tides nearly. At Melville Island, $\psi_3=45°$ nearly and R_3 is the greatest. The semester from midwinter to midsummer is also nearly expressed by

$$P+Q\sin \odot \text{ long. for } R_1.$$

Having obtained the empirical R and ψ, or A and a, any theoretic formula can be tested by the results.

April 29, 1852.

The EARL OF ROSSE, President, in the Chair.

The following communications were read:—

1. "Notes on the Impregnation of the Ovum in the Amphibia:" in a Letter to Thomas Bell, Esq., Sec. R.S. Communicated by Mr. Bell. Received April 27, 1852.

April 27, 1852.

MY DEAR MR. BELL,—During the month of March, now past, and since an abstract of my *Second Series* of observations "On the Impregnation of the Ovum in the Amphibia" has appeared in the Proceedings of the Royal Society for June 1851, I have ascertained that the spermatozoa of the Frog are not only brought into contact with the surface of the egg, in fecundation, as already known, but that some of these bodies penetrate into the thick gelatinous envelopes, as stated by Prevost and Dumas: and further, I have found that in those eggs which are completely fecundated, some spermatozoa have arrived at, and become partially imbedded in the internal envelope which encloses the yelk, although I have not yet been able to detect any within the yelk itself; nor have obtained any evidence of the existence of an orifice, or natural perforation in the external envelopes, through which these bodies might enter.

It is right that I should make this announcement without further delay, as the fact now mentioned necessitates some revision of my view respecting the nature of the impregnating influence, as expressed in the paper alluded to. This I propose to make, and to lay very soon before the Royal Society.

I remain, my dear Sir,
Yours very faithfully,
Thos. Bell, Esq., Sec. R.S. GEORGE NEWPORT.

2. "Further Experiments on Light." By Henry Lord Brougham, F.R.S., Member of the Institute of France, and of the Royal Academy of Sciences of Naples. Received March 5, 1852.

The author commences this account of his experiments by remarking, that "it is probable that some may consider the inference to be drawn from the following experiments as unfavourable to the doctrines of my former paper—I think I can explain the phenomena according to those doctrines—but be they ever so repugnant, we are of course in search of truth, and have no right even to wish that the balance may incline one way rather than another, far less to conceal any facts which may affect its inclination."

The leading experiment is this:—A speculum is placed in a beam of light and is inclined so that the reflected rays shall make a small angle with the surfaces. Near the speculum the axis of reflected rays coincides with that of the direct rays, but at a greater distance the two discs are separate. The speculum being placed horizontally across the pencil, coloured fringes appear both on the upper and lower side of the reflected disc. These two sets of fringes are alike in their colours and in the order of their colours, but the upper fringes are narrower than the lower, and they diminish in breadth with their distance from the disc, while the lower ones increase in breadth with their distance. If only one edge of the speculum is in the pencil there are only fringes on one side of the disc.

It appears that the breadth of the fringes is in some inverse proportion to the breadth of the speculum. When the speculum is a triangle with a very acute angle, the broadest fringes, and those most removed from the disc, answer to the points of the speculum where it is narrowest, and they increase regularly towards the point which answers to the acute angle or apex of the speculum. Their form is hyperbolic.

When the edges of the speculum are parallel, the disc near to it is filled with groups of fringes which vary in number, in breadth and in colour, at all the distances from the speculum. At one distance they form only a dark line running through the disc, and this is deep purple when examined closely. At a greater distance the fringes have other colours, and become broader again; and at a still greater distance they emerge into the shadow on both sides of the disc.

The phenomena of reflexion, it is stated, closely resemble those of flexion, as to the fringes, their colours, their magnitude, their variation at different distances from the bending edges, and at different distances of those edges from each other.

A convenient method of examining the variation of the fringes, whether of reflexion or of flexion, at various distances, is to incline the screen upon which they are received, so that it crosses the rays forming the fringes, which are exhibited upon it, at various distances from the edges. The line which each fringe describes being the projection of the line which the rays follow that form the fringe, we can in this manner observe if the course of these rays after flexion is rectilinear or curvilinear, the projection being, generally speaking, a line of the same kind with the original line; and at least never rectilinear if that original line is curvilinear.

If $y=f(x)$ be the line which the rays follow after flexion; ϕ the angle of the screen's inclination; $\frac{\sin\phi}{\cos\phi}=m$; and x^1 the abscissæ of the line of projection; then its equation is $y=f(\sqrt{1+m^2}\,.\,x^1)$. If the curve of the rays be supposed to be the equilateral conic hyperbola, the radius of curvature in the curve of projection, it is stated, must be less than that in the original line; and so the curvature is more easily discerned by the eye. As under no circumstances of inclination of the screen, and at no part whatever of the course of the fringes could the author perceive the least difference of form from all the other parts, he infers, either that the rays follow a rectilinear course, or that their deviation from it must be very small.

Though the phenomenon seem to indicate a crossing of the rays both in flexion and reflexion, at or near the distance at which the dark or deep purple line is formed, yet the author has never been able to observe that an obstacle placed between that point and the speculum (or the bending edges), made the fringes on the opposite side of the disc at the screen to disappear, but only the fringes on the same side with itself.

Referring to Fresnel's memoir, the author states that the principle laid down in it, "that the dilatation of the fringes depends solely upon the breadth of the aperture," will not afford an explanation of the phenomena described in his former paper respecting fringes formed by edges acting in succession, for he there showed that their breadth and their distances from the direct rays are in the inverse proportion of the distance of the edges; and if the edges are so placed that the rays pass parallel to each other, and not diverging, and the edges are moved to different distances in the same line, *e.g.* horizontally, then their distance from each other vertically being the same, the aperture is the same at all distances of the edges from each other horizontally, and yet the breadth of the fringes is inversely as the horizontal distance. Further, where the edges are not placed in succession, but directly opposite to each other, the breadths of the fringes do not appear to follow the exact inverse proportion of the distances of the edges (that is the size of the aperture), the observed breadths corresponding more nearly with the curve $y=\frac{m}{x}+\frac{m}{x^2}$, x being the distance of the edges, and y the breadth of the fringes.

The author considers that the internal fringes, or those of the

shadows of small bodies, called fringes of interference, require a more full examination than they have received in certain respects. As regards the central space and the two deep black fringes or intervals on each side of it, he remarks that no examination with a magnifier, and no inclination of the screen, at all resolves these colours into purple as in the dark line before described. They appear to follow a different law from that of the coloured ones as regards their breadths in proportion to their distances from the pin or other small object, at least if they are caused by interference, and if the effect of interference is inversely as the difference of the length of the rays; for that would give for the breadths the curve $y=\frac{m}{\sqrt{a^2+x^2}-\sqrt{b^2+x^2}}$, which nowise agrees with the admeasurements.

The action of transparent plates on the rays, in bending them, resembles in every respect that of opake plates, except that there being no shadow, the external fringes are not perceived. But the shadow of the edge of the plate is surrounded by two sets of fringes resembling exactly those surrounding the shadow of a hair or other small body placed upon the plate's edge, and following its course, with this only difference, that this shadow of the transparent plate's edge has no internal fringes as the hair or other small body's shadow has.

May 6, 1852.

The EARL OF ROSSE, President, in the Chair.

In compliance with the Statutes, it was announced from the Chair that the following Candidates are recommended by the Council for election into the Society:—

Arthur Kett Barclay, Esq.
Rev. Jonathan Cape.
Arthur Cayley, Esq.
Henry Gray, Esq.
Wyndham Harding, Esq.
Arthur Henfrey, Esq.
John Higginbottom, Esq.
John Mercer, Esq.
Hugh Lee Pattinson, Esq.
Rev. B. Price.
William Simms, Esq.
Hugh E. Strickland, Esq.
John Tyndall, Esq.
Nathaniel Bagshaw Ward, Esq.
Captain C. Younghusband, R.A.

A paper was read, entitled, "On Periodical Laws discoverable in the mean effects of the larger Magnetic Disturbances."—No. II. By Colonel Edward Sabine, R.A., Treas. and V.P.R.S. &c. Received March 18, 1852.

From the discussion of the magnetic observations made at Toronto and Hobarton in the years 1843, 44, 45, the author in a former paper adduced evidence of the existence of periodical laws by which the principal disturbances of the magnetic declination appeared to be regulated. Having since had occasion to examine the disturbances of the Declination at the same two stations in the three succeeding years 1846, 47, 48, he states that he had the satisfaction of

finding that the observations of these years confirm every deduction which he had ventured to make from the analysis of the disturbances of the former period; whilst new and important features have presented themselves in the comparison of the frequency and amount of the disturbances in *different years*, apparently indicating the existence of a *periodical variation*, which, either from a real or causal connection, or by a singular coincidence, corresponds precisely, both in period and epoch, with the variation in the frequency and magnitude of the solar spots, recently announced by M. Schwabe as the result of his systematic and long-continued observations.

The method pursued in examining the laws of the Declination-disturbances in 1846, 47, 48, is the same as that adopted in the three preceding years. Every hourly observation which was found to differ a certain amount from the mean value of the Declination in the same month and at the same hour was, as before, separated from the rest. The number of observations thus separated in the period commencing July 1, 1843, and ending July 1, 1848, was at Toronto 3940, and at Hobarton 3469, being respectively 1 in 9·43 at Toronto, and 1 in 10·55 at Hobarton, of the whole number of hourly observations. The disturbed observations being distributed into the several hours, months, and years in which they had occurred, their numbers and aggregate values in each particular hour, month, and year, were ascertained. They were then divided into easterly and westerly deflections, and the same process of distribution was gone through with each of the divisions. The *mean* hourly, monthly and yearly number and aggregate values in the whole period were then taken as the respective units, and the ratios to these units computed for each of the hours, months and years; whereby the relations, whether of numbers or of aggregate values in different hours, different months, and different years, were shown.

The results thus obtained are discussed separately in the following order:—

I. Inequality or variation in the number and aggregate values of the disturbed observations in *different hours*. This examination is made by classing together—1st, easterly disturbances at Toronto and westerly at Hobarton; and 2nd, westerly at Toronto and easterly at Hobarton.

From the first classification, it appears that at both stations there are fewer disturbances, and their aggregate values are less in the hours of the day than in those of the night; that 9 P.M. is the hour of the maximum of frequency and also of value at Toronto, and 11 P.M. at Hobarton; and that the periods of minima are between 2 and 3 P.M. at Toronto, and between 5 and 6 A.M. at Hobarton. It appears further that the average value has a similar law of variation to that of the number and aggregate value.

The second classification shows that at Hobarton the contrast both in frequency and aggregate value is still between the hours of the day and those of the night, the ratios being, however, in this case greater than unity during the former hours, and less than unity during the latter, contrary to what takes with the easterly disturb-

ances: at Toronto the contrast is between the hours from noon to midnight, and those from midnight to noon, the ratios being greater than unity during the latter hours, and less than unity during the former. In both cases the variation in the ratios appears to be dependent on the hours of *local*, not on those of absolute time.

From a table showing the ratios of easterly aggregate values to westerly at Toronto, and of westerly to easterly at Hobarton, it appears that, at both stations, the deflection (due to disturbance) of the end of the magnet of the same name as the magnetic latitude is to the west during the hours of the day or from 5 A.M. to 5 P.M.: at a little before 6 P.M. at Toronto, and a little after 6 at Hobarton, the deflections pass through zero (or the undisturbed position of the magnet) into easterly deflections of that end. The magnitude of those deflections rapidly augments to a maximum at 9 P.M. at Toronto, and at 10 P.M. at Hobarton; they again pass through zero between 4 and 5 A.M.; and attain the westerly maximum at 7 A.M., the variation in the magnetic direction due to the disturbances depending, like those of number and value, on the hour of local time.

II. Inequality or variation in the number and aggregate values of the disturbed observations in *different months*. From the tables which are given, it is obvious that there is a systematic variation in the numbers and aggregate values of the disturbances in the different months; and at both stations the easterly and westerly ratios, separately considered, differ little in the characters which they assign to the variation, from the ratios of the two combined. The most distinctly marked feature is that the disturbances are less frequent and have a less aggregate value in November to February at Toronto, and in May to August at Hobarton, than in the other months respectively: so that the disturbances are governed by a law depending either on the period of the year, or on *local* season, not on absolute time.

III. Variation in the number and aggregate values of the disturbed observations in *different years*. Taking the ratios of the numbers and aggregate values of the disturbed observations at Toronto and Hobarton in the different years (from 1843 to 1848), to the average annual number and aggregate value respectively, it appears that there is a remarkable correspondence in the variation of these ratios in different years at the two stations; and that at each, both ratios increase progressively from 1843 to 1848, with the single exception of 1845, in which there is a small diminution in that of the number and also that of the value. Taking the mean of the ratios at Toronto and Hobarton, the ratio of the number increases from 0·60 in 1843 to 1·43 in 1848, and the ratio of the value from 0·52 in 1843 to 1·51 in 1848, the variation in each having much more the aspect of a *periodical inequality* than of an accidental variation. Looking to the theoretical importance of the existence of a periodical inequality of this nature, affecting at the same time, and in the same manner, parts of the globe most remote from each other, the author refers to the confirmation it may obtain from contempora-

neous observations at other stations. Pending such confirmations he remarks that this progressive increase in the amount of disturbance at Toronto and Hobarton, between the years 1843 and 1848, derives great additional interest and importance from its apparent connection with an equally remarkable progressive increase which took place at the same two stations, in the magnitude of the diurnal range of the Declination in the same years. From the mean magnitude of the diurnal variation of the Declination in each month, tables are deduced showing the mean magnitude or ranges in the four months constituting the respective seasons, and in the twelve months constituting the year, in each year from 1843 to 1848, both at Toronto and at Hobarton. From these tables it appears that at each station, for each of the seasons and for the whole year, the diurnal range of the Declination had a progressive increase during that period; the increase for the whole year being from 8′·90 in 1843 to 12′·04 in 1848 at Toronto, and from 7′·66 to 11′·43 at Hobarton. In support of the opinion that these progressive increases in the range of the diurnal variation at two stations separated from each other by nearly half the surface of the globe are independent and corresponding measures of a general phenomenon, the author adduces the results obtained by Dr. Lamont from the observations at Munich. From these it appears that the mean range of the diurnal variation in monthly periods at Munich increased progressively from 7·82 in 1843 to 11·15 in 1848.

The author remarks that the increase so distinctly marked in the two classes of phenomena between the years 1843 and 1848 tends to indicate a causal connection subsisting between the disturbances and the regular diurnal variation. If we suppose the diurnal variation to be divided into two portions, one of which is nearly uniform in amount throughout the year (at the same station), whilst the other has a hemispherical phase, developed in either hemisphere according as the sun is in the northern or the southern signs,—it is the former of these two portions which sustains the variation consistent with and apparently related to the variation in the number and values of the disturbances.

That the progressive increase in the mean monthly diurnal range, from 1843 to 1848, was not confined at Toronto and Hobarton to the Declination only, but took place likewise in the diurnal variations of the Inclination and Total Force, is shown by the tables which are given.

In conclusion the author observes, that "in our present ignorance of the physical agency by which the periodical magnetic variations are produced, the possibility of the discovery of some cosmical connection which may throw light on a subject as yet so obscure should not be altogether overlooked. As the sun must be recognised as at least the *primary* source of all magnetic variations which conform to a law of local hours, it seems not unreasonable that in the case of other variations also, whether of irregular occurrence or of longer period, we should also look in the first instance to any periodical variation by which we may learn that the sun is affected, to see

whether any coincidence of period or epoch is traceable. Now the facts of the solar spots, as they have been recently made known to us by the assiduous and systematic labours of Schwabe, present us with phenomena which appear to indicate the existence of some periodical affection of an outer envelope, or photosphere, of the sun; and it is certainly a most striking coincidence that the period, and the epochs of maxima and minima, which M. Schwabe has assigned to the variation of the solar spots, are absolutely identical with those which have been here assigned to the magnetic variations." From the results of his observations of the solar spots from the years 1826 to 1850, M. Schwabe has derived the conclusion that "the numbers in the table leave no room to doubt that, at least from the years 1826 to 1850, the solar spots have shown a period of about ten years, with maxima in 1828, 1837, and 1848, and minima in 1833 and 1843." M. Schwabe has not been able to derive from the indications of the thermometer or barometer any sensible connection between climatic conditions and the number of spots. The same remark would of course hold good in respect to the connection of climatic conditions with the magnetic inequalities, as their periodical variation corresponds with that of the solar spots. But it is quite conceivable that affections of the gaseous envelope of the sun, or the causes occasioning those affections, may give rise to sensible magnetical effects at the surface of our planet, without producing sensible thermic effects.

May 13, 1852.

WILLIAM SPENCE, Esq., V.P., in the Chair.

The following communications were read:—

1. "Report of the general process adopted in Graduating and Comparing the Standard Meteorological Instruments for the Kew Observatory." By Mr. John Welsh. Communicated by Col. Sabine on the part of the Committee of Recommendations of the Government Grant. Received May 6, 1852.

In offering to the Committee a short statement of the progress made at the Kew Observatory in the construction and verification of thermometers, I shall first describe generally the method pursued in the graduation of standard instruments.

The plan of operations hitherto adopted has been that proposed by M. Regnault, and consists essentially of the following steps:—1st. Calibration of the tube: 2nd. Graduation of the scale: and 3rd. The determination of the scale coefficients.

1. *Calibration.*—A tube having been selected as being tolerably free from all visible defects, a short column of mercury, generally less than one inch in length, is introduced. The tube is then attached to the frame of Perreaux's dividing-engine, and by means of flexible tubing is put in connection at both ends with india-rubber bags, the pressure upon which can be regulated by means of screws. The

mercury is then brought to the part of the tube where the graduation is proposed to commence. The cutting-frame of the engine carries also a small microscope with cross wires in its focus; on turning the dividing-screw, the microscope-wire is brought to coincide with the first end of the mercury, and the screw is then turned forward until the wire reaches the second end; the length of the column is thus given in revolutions of the screw. By means of the india-rubber bags, the mercury is made to move along the tube until the first end coincides again with the microscope-wire; the length of the column is again measured, and the mercury again moved forward; the same process being repeated until the column has been measured for each length of itself through the whole extent of the proposed scale. Permanent marks are made on the glass at the points of commencement and ending of the calibration. If the progress of the numbers shows any considerable irregularity in the tube, and as a verification of the first set of measures, it is well to repeat the calibration, commencing in this case at a point one-half the length of the column in advance of the original starting-point. A series of measures interpolated from the two sets may then be adopted. Some experience is necessary in order to bring with facility the end of the mercury exactly to the wire of the microscope; but when care is taken to use very pure mercury and clean tubes, the operation can generally, after a little trouble, be accomplished with much accuracy. M. Regnault, I believe, recommends that the motion of the mercury should be regulated by the breath, a drying substance being interposed to prevent moisture entering the tube. This method was employed for some of the first instruments made at Kew, but was abandoned in favour of the elastic bags.

2. *Graduation.*—The measured lengths of the column of mercury in its successive steps along the tube correspond to equal volumes. Assuming that the calibre of the tube does not vary throughout the small length of the calibrating column, if we divide the spaces occupied successively by the mercury into an equal number of parts, it is evident that the divisions will represent the same *capacity*, although they may be of very different *lengths*. Before making the tube into a thermometer, the divisions of the scale may be verified by introducing a longer column of mercury, and examining whether the column occupies an equal number of divisions in different parts of the scale. If there should be any irregularity, a table of corrections may readily be formed. It will generally be found, however, that if the operations have been performed with care, and the tube is not very faulty, no correction will be necessary. The divisions are cut with a fine needle-point upon a coating of engravers' varnish, and afterwards etched with fluoric acid. The required dimensions of the bulb may be found approximately by weighing a measured length of the mercurial column, and from the known expansion of mercury and its specific gravity computing the capacity of the bulb.

3. *Determination of the scale coefficient.*—The thermometer having been filled with mercury, we have an instrument the divisions of whose scale represent equal increments of the volume of the fluid,

but are entirely of an arbitrary value. If now we determine the points of the scale at which the mercury stands in freezing and boiling water, we can immediately convert the arbitrary scale-readings into degrees of the ordinary scales of temperature. If a be the scale-reading for the freezing-point, and b that for the boiling-point, the temperature by Fahrenheit's scale corresponding to any reading $n = \left(\frac{n-a}{b-a}\right) 180 + 32$. The freezing-point is determined by placing the thermometer in finely-pounded ice, from which the water is drained off as it melts. The boiling-point is ascertained by the form of apparatus employed by M. Regnault; the temperature observed is that of steam, whose elasticity is the same as that of the atmosphere. A small siphon water-gauge communicating with the interior of the vessel gives notice to the observer when the ebullition is being carried on too rapidly. The steam is generated from distilled water. The height of the barometer is observed at the time of the experiment, and the correction to a uniform height of 30 inches (reduced to 32°) is found from Regnault's table. In determining the fixed points, the stems of the thermometers are kept vertical; if the subsequent comparisons with other instruments are made in the same position, no error will arise from the expansion of the bulb caused by the pressure of the column of mercury. If, however, the thermometers are intended to be used in any other than a vertical position, it becomes necessary to determine the fixed points also in a horizontal position.

In accordance with the plan here sketched, fifteen thermometers have been completed with arbitrary scales. About thirty more tubes have been calibrated, and the bulbs attached and filled, but the scales not yet divided. The principal object in graduating the tube with an arbitrary scale is the convenience it affords of testing the divisions before it is converted into a thermometer. It is now proposed to divide the scale at once into Fahrenheit degrees after the thermometer has been made, and to test the accuracy of the divisions afterwards by detaching a portion of the mercurial column and making it move along the tube. If the scale should not then be found correct, a table of its errors can be formed and furnished with the instrument, or the thermometer rejected. The scales of these thirty thermometers have not yet been proceeded with, as it is desirable, before doing so, to allow the freezing-point to have attained a permanent position. A few divisions have been cut on the tubes near the freezing-point, and the reading with reference to this short arbitrary scale taken from time to time in melting ice. The period elapsed since the construction of the thermometers has been too short to afford as yet much information as to the probable constancy of the freezing-points. They have, however, already shown generally a tendency to rise, in some cases to the extent of nearly $0^{\circ}\cdot 3$ Fahr., but in most of them it does not yet exceed $0^{\circ}\cdot 1$ or $0^{\circ}\cdot 2$. Another peculiarity in connection with the freezing-point has shown itself in almost all the thermometers yet tried. After a thermometer has been exposed for some weeks to the ordinary tem-

perature of the air, if its freezing-point be ascertained, and it be then suddenly exposed for a short time to the temperature of boiling water, and again immediately placed in ice, it is found that the latter determination of the freezing-point will be *lower* than the former by a very appreciable amount, generally between 0°·1 and 0°·2 Fahr. The freezing-point does not recover its previous position for some time, probably two or three weeks. This peculiar displacement of the freezing-point has been found to take place also in the case of a standard by Troughton and Simms belonging to the Royal Society. The freezing-point of this instrument, before being raised to the temperature of boiling water, was 32·25, afterwards it had fallen to 32·15. This displacement of the freezing-point has been remarked by Mr. Sheepshanks in the course of his experiments on standard thermometers*. From the experiments now in progress, it is to be hoped that, after a time, some approximation may be made to the laws of these perplexing phænomena.

The apparatus employed for comparing the indications of different thermometers, consists of a cylindrical glass vase 15 inches deep and $8\frac{1}{2}$ inches in diameter,—a stand for supporting the thermometers under comparison, and a means of agitating the water in such a way as completely to assimilate the temperature throughout the vessel. The stand for the thermometer is a vertical rod, supported by a small tripod resting on the bottom of the vase. The thermometers are suspended from hooks sliding on this rod, and adjustable to any height; they are arranged, with their bulbs at the same height in a circle 3 inches diameter round the rod, and kept fixed with sufficient firmness below by being strapped with elastic bands against a projecting six-rayed frame attached to the supporting rod. Six thermometers of almost any form and length can thus be compared at once. The agitator is a flat ring of tinned iron, about 2 inches broad, fitting easily within the vase, and connected by four light rods with a similar ring at top, which serves as a handle. A packing of india-rubber is placed on the outer rim of the plunger to prevent jarring against the glass. The flat tin ring is cut half across at several places, and the corners bent in various ways, so that when moved upwards and downwards the water is driven in *all* directions. The dimensions of the agitator are so arranged, that no part of it can possibly touch the thermometers when in operation. The vase, containing water, the stand with thermometers, and the agitator, are mounted upon a wooden revolving stand. The depth of water in the vase is always sufficient to include the whole of the column of mercury, the scales being observed through the water. In taking the observations, the observer, after agitating the water briskly for some time, turns the revolving stand till each thermometer is brought successively opposite to his eye, reading off the scales as quickly as possible to an assistant, who writes down the numbers. Proceeding in this way, I find that six thermometers can be read off and recorded easily in 20 seconds. It is of course desirable to make more than

* This fact, I find, is also mentioned in Faraday's "Chemical Manipulation," edit. 1827, p. 139.

one set of readings for each temperature; and in order to avoid as much as possible the changes which may occur during the reading off, it is well to reverse the order of observing the instruments, that is, to read them alternately in the order one to six, and six to one.

The following table contains the results of comparisons of six thermometers, and will show the accuracy which may be obtained by the method of comparison just described; it will also exhibit the accordance in the indications of instruments graduated according to Regnault's process. Each result is the mean of six comparisons. No optical assistance was used in reading off the scales. The freezing-points of all the instruments were determined on the same day, after the comparisons were made.

Results of Comparisons of various Thermometers, March 19, 1852.

Standard Thermometers.							Barrow, E.I.C., S 7, No. 4.		Newman (Makerstoun).		Troughton and Simms (Royal Society).	
Kew No. 4.		Kew No. 14.		Fastré 231 (Regnault).		Temperature from mean of standards.						
Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.		Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.	Observed temperature.	Diff. from mean of standards.
°	°	°	°	°	°	32°·00	32°·05	+0°·05	32°·05	+0°·05	32°·25	+0°·25
38·69	−0·02	38·73	+0·02	38·72	+0·01	38·71	38·91	+0·20	38·86	+0·15	38·96	+0·25
45·05	+0·01	45·03	−0·01	45·03	−0·01	45·04	45·30	+0·26	45·18	+0·14	45·30	+0·26
49·96	0·00	49·97	+0·01	49·96	0·00	49·96	50·34	+0·38	50·23	+0·27	50·23	+0·27
55·33	−0·02	55·35	0·00	55·37	+0·02	55·35	55·87	+0·52	55·75	+0·40	55·62	+0·27
60·07	+0·01	60·06	0·00	60·05	−0·01	60·06	60·65	+0·59	60·58	+0·52	60·34	+0·28
65·39	−0·01	65·39	−0·01	65·41	+0·01	65·40	65·99	+0·59	66·03	+0·63	65·65	+0·25
69·93	0·00	69·92	−0·01	69·95	+0·02	69·93	70·57	+0·64	70·67	+0·74	70·22	+0·29
74·69	0·00	74·68	−0·01	74·69	0·00	74·69	75·39	+0·70	75·54	+0·85	75·02	+0·33
80·08	+0·02	80·03	−0·03	80·06	0·00	80·06	80·78	+0·72	81·00	+0·94	80·44	+0·38
85·30	−0·01	85·30	−0·01	85·33	+0·02	85·31	86·10	+0·79	86·25	+0·94	85·75	+0·44
90·50	0·00	90·49	−0·01	90·51	+0·01	90·50	91·36	+0·86	91·47	+0·97	90·87	+0·37
95·29	+0·04	95·23	−0·02	95·24	−0·01	95·25	96·15	+0·90	96·32	+1·07	95·72	+0·47
101·78	+0·01	101·76	−0·01	101·77	0·00	101·77	102·71	+0·94	103·04	+1·27	102·26	+0·49
109·21	+0·05	109·11	−0·05	109·15	−0·01	109·16	110·08	+0·92	110·62	+1·46	109·58	+0·42
						212·00					212·47	+0·47

The thermometers "Kew No. 4" and "Kew No. 14," were graduated on the stems by myself with arbitrary scales: the bulb of No. 4 is spherical, and is about $\frac{3}{4}$ inch diameter; that of No. 14 is cylindrical, $\frac{3}{4}$ inch long and $\frac{1}{4}$ inch diameter, and very sensitive. "Fastré No. 231 (Regnault)" is a standard by Fastré of Paris, also graduated on the stem with an arbitrary scale according to Regnault's process. This instrument was examined and approved by M. Regnault; the determination by him of the scale coefficient agreed closely with that afterwards made at Kew. The bulb is cylindrical, about $1\frac{1}{2}$ inch long and $\frac{1}{4}$ inch diameter. "Barrow, E.I.C., S 7, No. 4," is one of a number of thermometers made for the East India Company and sent to Kew for examination. Its scale

is of brass divided to degrees. "Newman (Makerstoun)" is the instrument which was supplied to the Makerstoun Observatory as a standard, and to whose indications the results of the temperature observations made there since 1841 have been "corrected." It was, at my suggestion, sent to Kew by Sir Thomas Brisbane for comparison with our standards. "Troughton and Simms (Royal Society)" is a standard belonging to the Royal Society. As its scale extends to above 212, its boiling-point was examined in the same apparatus employed for the Kew standards, its brass scale remaining attached to the tube. It was found to read 212°·7 when the barometer, reduced to 32°, stood at 30·136 inches.

The errors of a thermometer which has been already carefully examined between 32° and about 100°, may be obtained with considerable accuracy for temperatures below 32°, without using a freezing mixture, by the following process. Detach from the column of mercury a portion which will occupy about 40 or 50 degrees of the scale: bring this column within the known part of the scale. Let a, b be the readings at the upper and lower ends respectively; α, β the index errors at these points as determined by comparison with a standard. Move the column until its lower end coincides with some degree below 32°, the upper end being within the compared portion of the scale. Let c, d be the scale-readings for the upper and lower ends in the new position, γ being the scale error corresponding to c. The error of the scale at d will then be

$$d-\{\overline{c-\gamma}-(\overline{a-\alpha}-\overline{b-\beta})\}.$$

The true length of the detached column may be obtained with increased accuracy by taking a mean of several measures within the known part of the scale. This method was adopted for "Newman (Makerstoun)" and "Troughton and Simms (Royal Society)," and the following errors obtained:—

Newman (Makerstoun). Temperature.	Error.	Troughton and Simms (R.S.). Temperature.	Error.
0°·7	−0°·05	5°·1	+0°·14
6·2	−0·08	10·0	+0·17
10·7	−0·12	15·0	+0·16
14·6	−0·10	20·0	+0·16
20·2	−0·04	24·8	+0·16
25·8	0·00		

The error of "Newman" had been previously found, by comparing with a standard in a freezing mixture at −3°, to be inappreciable.

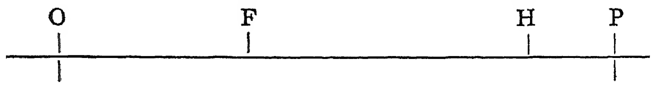
Mr. Welsh's Report, No. 2.

"On the Graduation of the Thermometers supplied from the Kew Observatory for the use of the Arctic Searching Expedition under Sir Edward Belcher."

These instruments were twelve in number, seven mercurial and five spirit thermometers, graduated for low temperatures. The pro-

cesses adopted for the two kinds of instruments being different, I shall describe them separately.

I. *Mercurial Thermometers.*—These were divided into degrees of Fahrenheit's scale in the following manner:—The tube was first calibrated in the way already described in my former report on the construction of thermometers (dated March 22); marks being made on the tube at each extremity of the calibrated space. The bulb was then made and the mercury introduced by the glass-blower, the dimensions of the bulb and the quantity of fluid being made as nearly as possible to correspond with the scale proposed to be made. The thermometer was then placed in melting ice and the freezing-point approximately set off with an ink mark; a similar mark being also made for a temperature of between 95° and 100°. A short arbitrary scale of four or five divisions was then divided at each of those points. The thermometer was then again placed in ice and the freezing-point determined accurately with reference to the lower short scale; and comparisons with two standard thermometers in water determined the value of the upper short scale. Let OP be the calibrated portion of the tube, O being the point of commencement, F the



freezing-point as determined by means of the short arbitrary scale, and H the higher point of the scale similarly obtained. Let the distances OF, FH, HP be measured by the screw of the dividing engine. Let R_1, R_2, R_3, &c., R_f, &c., R_h, &c. be the lengths, in revolutions of the dividing screw, of the calibrating column of mercury for each successive step in its progress along the tube during the process of calibration; R_f being the length of the step in which the point F occurs, and R_h that in which H occurs. The values of R_1, R_2, &c. have been registered in the process of calibration; OF and OH have been obtained independently; the second measurement of OP, when compared with the sum of all the R's, will show with what exactness* the column of mercury has been passed through its own length in its progress along the tube. Let r_f be the number of revolutions between the first end of the step f and the point F, and similarly r_h for the step h. We have then

$$\mathrm{OF} = R_1 + R_2 + R_3 + \&c. + R_{f-1} + r_f$$

$$\text{and } \mathrm{OH} = R_1 + R_2 + \; \dots\dots \; + R_{h-1} + r_h;$$

whence we obtain r and r_h. Let K be the number of degrees equivalent to one length of the calibrating column,—this being of course constant for each length along the tube on the supposition of equal increments of volume for equal increments of temperature.

* If this should be found slightly in error, it can produce no appreciable error in the graduation, as an error could only arise from the alteration of the tube's capacity, which might take place in a length equal to the difference found, this difference being in perhaps no case more than $\frac{1}{100}$ or $\frac{1}{50}$ inch.

Also, if we suppose that the capacity of the tube does not vary throughout the length of a single calibrating step, $\frac{r_f}{R_f}$ and $\frac{r_h}{R}$ will give the fractional parts of a step by which the points F and H are respectively in advance of the first ends of the steps f and h. We have then

$$OF=\left(\overline{f-1}+\frac{r_f}{R_f}\right)K, \quad OH=\left(\overline{h-1}+\frac{r_h}{R_h}\right)K;$$

and $FH=\left(h-f+\frac{r_h}{R_h}-\frac{r_f}{R_f}\right)K=T_h$, the higher temperature$-32°$;

whence $K=\dfrac{T_h}{h-f+\frac{r_h}{R_h}-\frac{r_f}{R_f}}$. The degree corresponding to the point O is $32°-\left(\overline{f-1}+\frac{r_f}{R_f}\right)K$. The length of one degree for any individual step x is $\frac{R_x}{K}$.

From the quantities thus obtained, a table may readily be formed showing the value in revolutions of the dividing screw of one degree at all parts of the scale, and the graduation may then be proceeded with accordingly. The graduation is carried from $-40°$ to $+120°$ or $130°$ Fahr.

II. *Spirit Thermometers.*—In the graduation of mercurial thermometers, the practice is to consider the increments of volume to be proportional to increments of temperature. If this were assumed in the case of spirit thermometers, very serious errors would be the result, even within moderate ranges of temperature. Dr. Miller having considered alcohol, as on the whole, the best fluid for thermometers intended to measure very low temperatures, I was supplied by him with some which he had himself prepared with great care, its specific gravity being 0·796 at 60° Fahr. The first step to be taken was the determination of the *law* of expansion of the fluid in glass, as compared with that of mercury. For this purpose a tube was calibrated and divided with an arbitrary scale according to Regnault's process: its divisions were found, upon verification, to be of exactly equal capacity throughout. The tube was then furnished with a bulb of the same dimensions as those intended to be supplied to the Admiralty, and filled with the alcohol. This thermometer was marked S. 9 E. Comparisons were then made between the readings of this instrument and those of a standard mercurial thermometer, through as large a range of temperature as was found practicable. The comparisons above the freezing-point were taken in water, in the apparatus described in my former report; those below 32° were taken in freezing mixtures of ice and salt or chloride of calcium. The following Table contains the results of two series

of experiments; the numbers in the first two columns are differences from the freezing-point; those in the first being Fahrenheit's degrees; and in the second and third columns, the arbitrary scale divisions of the spirit thermometer S. 9 E.

Table, containing results of comparisons between a Standard Mercurial Thermometer, and a Spirit Thermometer with an arbitrary scale of uniform capacity.

First Series.			Second Series.		
Standard mercurial thermometer.	Spirit thermometer S. 9 E.	S. 9 E. Observed *minus* calculated.	Standard mercurial thermometer.	Spirit thermometer S. 9 E.	S. 9 E. Observed *minus* calculated.
°	Scale div.	Scale div.	°	Scale div.	Scale div.
+69·95	+209·5	+0·2	+65·76	+196·4	+0·2
+66·93	+199·7	−0·1	+60·04	+178·3	0·0
+53·15	+156·7	−0·3	+52·04	+153·5	−0·1
+40·53	+118·2	−0·3	+37·72	+110·3	+0·2
+20·83	+ 60·1	+0·2	+24·05	+ 69·8	+0·4
+17·80	+ 51·0	−0·1	+16·01	+ 46·7	+0·8
−18·44	− 50·5	+0·9	−16·38	− 44·8	+0·9
−36·15	− 98·0	+1·2	−29·00	− 79·7	+0·4
−43·14	−117·9	−0·2	−36·33	−100·2	−0·5
			−44·72	−123·0	−1·1

To deduce the law of expansion from these comparisons, the numbers were arranged in equations of the form

$$AT+BT^2-N=0, \quad \ldots\ldots\ldots\ldots \quad (1.)$$

where T is the number of Fahrenheit's degrees from 32°, N the corresponding number of divisions by thermometer S. 9 E., A and B being the constants whose value is to be ascertained: the constants depending on higher powers of T than the second, were not considered.

The values of A and B were obtained from the equations by the method of least squares, and were as follows:—

From first series .... A=2·8203 ...... B=0·002455
From second series .... A=2·8377 ...... B=0·002221
The mean of both series giving .. A=2·829 ...... B=0·002338

The numbers in the columns "Observed *minus* calculated," are obtained by taking the difference between the observed readings of the spirit thermometer, and the numbers calculated from the *mean* values of A and B just stated.

Having determined upon the adoption of the law of expansion stated above, the graduation of the spirit thermometer was proceeded with as follows.

The process of calibrating the tubes was the same as for the mercurial thermometers; as in these, also, the freezing-point and a temperature of 90° or 95° were determined with reference to short scales on the stems; the distances OF, OH (figure, page 184) were also measured; and by comparing these measurements with the

numbers obtained by calibration, they were expressed in terms of lengths of the calibrating column.

The equation (1.) may be put under the form $N=A(T+\theta T^2)$ by making $\theta=\frac{B}{A}$. Let f and h be the distances OF, OH expressed in steps of the calibrating column; $FH=h-f$. Let T_h be the number of degrees above 32° corresponding to H, and let α_0 be the value, in terms of a calibrating step, of one degree at the temperature 32°: we have then, according to the fundamental equation (1.),

$$h-f=\alpha_0\,(T_h+\theta T_h^2) \text{ or } \alpha_0=\frac{h-f}{T_h+\theta T_h^2}.$$

We may in general, without sensible error, assume that the value of one degree is uniform throughout the length of a single calibrating step, or if the column of mercury has been rather too long, we may subdivide the steps by interpolation. From the value of α_0, now obtained, we can find with sufficient exactness the temperature corresponding to the middle of the step f. It will now be convenient to make use of a table, derived from the values of A and B, showing the relative lengths of one degree at different temperatures on the supposition of uniform capacity of the tubes. The following are the values for every ten degrees, from −70° to +100° Fahr.:—

Temp. Fahr.	λ.	Temp. Fahr.	λ.
°		°	
−70	0·831	+20	0·980
−60	0·848	30	0·997
−50	0·864	40	1·013
−40	0·881	50	1·030
−30	0·897	60	1·046
−20	0·914	70	1·063
−10	0·930	80	1·079
0	0·947	90	1·096
+10	0·964	100	1·112

The value in degrees of the step $f=\frac{1}{\alpha_f}=K_f$. Then calling the numbers in the table λ, since $\frac{\alpha_0}{\alpha_f}=\frac{\lambda_0}{\lambda_f}$, we find $K_f=\frac{1}{\alpha_0}\cdot\frac{\lambda_0}{\lambda_f}=\frac{K_0}{\lambda_f}$. This gives us the temperature corresponding to each end of the step f, and we may then proceed in like manner to find the values of the neighbouring steps, and so obtain successively the values throughout the whole range of the thermometer. The temperature corresponding to the point O in the figure is found by subtracting the sum of all the values of K between O and F from 32°. The length, in turns of the dividing screw, for any degree x is $\frac{R_x}{K_x}$, where R is the length of the step in which x occurs, and K_x the equivalent number of degrees. A table can then be constructed, showing the lengths of each successive degree, commencing from the point O, by the aid of which the graduation may be performed. The scales extend to −75° Fahr.

The time at my disposal was scarcely sufficient to test the thermometers supplied to the Arctic Expedition so completely as I should have wished. The mercurial thermometers were after their graduation compared incidentally at two or three different temperatures, and found to agree generally to 0°·1 Fahr. They were all placed in melting ice, when it was found that four of them read exactly 32°, the other three, viz. Nos. 34, 46, 47, were about 0°·1 too low. In a few of these thermometers the column of mercury could be readily broken: when this column was moved to different portions of the scale, it was found to occupy precisely the same number of divisions. This was the case with four of the instruments; the other three not having been tested in this way.

The five spirit thermometers were compared at four different temperatures with a standard mercurial thermometer. The comparison at 0° being taken in ice and salt, is not very trustworthy. Their errors were as follows:—

Temp. by mer. stand.	S. 2.	S. 4.	S. 6.	S. 7.	S. 8.	Mean of errors.
°	°	°	°	°	°	°
65	+0·8	−0·3	−0·2	+1·3	−0·1	+0·30
52	+0·8	−0·2	−0·3	+1·4	0·0	+0·34
32	+0·8	−0·1	−0·3	+1·4	−0·3	+0·30
·0	+0·6	0·0	0·0	+1·7	+0·2	+0·50

The numbers in the column "Mean of errors" seem to indicate little error of a systematic nature. In the case of Nos. 2 and 7, the index error is very large: this, it is believed, is owing to some of the vapour of alcohol having become condensed in the upper portion of the tube before the fixed points were determined, and having escaped my notice; in fact the greatest attention is required to avoid errors from this source. These spirit thermometers cannot by any means be considered as standard, although they are doubtless more trustworthy than most of those usually made. The limited time at my command for the completion of the instruments, prevented the possibility of rectifying any blunders into which I might have fallen, owing to my inexperience in such work, and the intricacy of the problem.

John Welsh.

Kew Observatory, April 21, 1852.

2. The Reply of the President and Council to a Letter addressed to them by the Secretary of State for Foreign Affairs, on the subject of the cooperation of different Nations in Meteorological Observations. Communicated by direction of the President and Council.

Somerset House, 10th May 1852.

Sir,—I have the honour to acknowledge the receipt of your letter of March the 4th, transmitting, by direction of the Earl of Malmesbury, several documents received from foreign governments in reply to a proposal made to them by Her Majesty's Government, for their cooperation in establishing a uniform system of recording meteorological observations, and requesting the opinion of the Pre-

sident and Council of the Royal Society in reference to a proposition which has been made by the Government of the United States, respecting the manner in which the proposed cooperation should be carried out.

Having submitted your letter with its enclosures to the President and Council of the Royal Society, I am directed to convey to you the following reply.

With reference to the subject of well-directed and systematically conducted meteorological observations generally, and to the encouragement and support to be given to them by the governments of different countries, the President and Council are of opinion that they are highly deserving of much consideration, not only for their scientific value, but also on account of the important bearing which correct climatological knowledge has on the welfare and material interests of the people of every country.

With reference to the proposal for the establishment of a uniform plan in respect to instruments and modes of observation, the President and Council are not of opinion that any practical advantage is likely to be obtained by pressing such a proposition in the present state of meteorological science. Most of the principal governments of the European Continent, as Russia, Prussia, Austria, Bavaria and Belgium, have already organized establishments for climatological researches in their respective states, and have placed them under the superintendence of men eminently qualified by theoretical and practical knowledge, and whose previous publications had obtained for them a general European reputation. Such men are Kupffer, Dove, Kreil, Lamont and Quételet; under whose direction the meteorological observations in the above-named countries are proceeding; the instruments have been constructed under their care, and the instructions drawn up and published by them under the sanction of their respective governments. The observations as they are made are sent to them, are reduced and coordinated under their superintendence, and are published at the expense of the governments. Every year is now producing publications of this nature in the countries referred to, and by the rapid intercommunication of these, the results of the experience of one country and the modifications and improvements which experience may suggest, become quickly known to all. To call on countries already so advanced in systematically conducted meteorological observations to remodel their instructions and instruments, with a view of establishing uniformity in these respects, would probably, if pressed, elicit from other governments also the reply which Her Majesty's Government have received from Prince Schwarzenberg, conveyed in the Earl of Westmoreland's letter to Viscount Palmerston, viz. the transmission of a copy of the instructions which have been given to the Meteorological Observatories, forty-five in number, in the Austrian dominions, and a reference to the results obtained at those observatories, which are stated to be in regular course of publication.

In an earlier stage, when these establishments were either forming or were only in contemplation, it was considered that advantage

might arise from a discussion of the objects to be principally kept in view, and of the instruments and methods by which these might be most successfully prosecuted. For this purpose, a conference was held at Cambridge, in England, in 1845, which was attended by many of the most distinguished Meteorologists in Europe, and amongst them by all the gentlemen whose names are above stated, and who were expressly sent by their respective governments. The impulse communicated by this assemblage was without doubt highly beneficial, and the influence of the discussions which took place may perhaps be traced in some of the arrangements under which the researches in different countries are now proceeding; but in the stage to which they have advanced, it may be doubted whether any measures are likely to be more beneficial than those which would increase the facilities of a cheap and rapid intercommunication of the results of the researches which are in progress.

With reference "to the suggestions made by the scientific men of the United States," the proposition of Lieutenant Maury, to give a greater extension and a more systematic direction to the meteorological observations to be made at sea, appears to be deserving of the most serious attention of the Board of Admiralty. In order to understand the importance of this proposition, it will be proper to refer to the system of observations which has been adopted of late years in the navy and merchant service of the United States, and to some few of the results to which it has already led. Instructions are given to naval captains and masters of ships, to note in their logs the points of the compass from which the wind blows, at least once in every eight hours: to record the temperature of the air, and of the water at the surface, and when practicable, at considerable depths of the sea: to notice all remarkable phenomena which may serve to characterize particular regions of the ocean, more especially the direction, the velocity, the depths and the limits of the currents: special instructions also are given to whalers, to note down the regions where whales are found, and the limits of the range of their different species. A scheme for taking these observations regularly and systematically, was submitted by Lieut. Maury to the Chief of the Bureau of Ordnance and Hydrography, in 1842, and instantly adopted: detailed instructions were given to every American shipmaster, upon his clearing from the Custom House, accompanied by a request that he would transmit to the proper office, after his return from his voyage, copies of his logs, as far at least as they related to these observations, with a view to their being examined, discussed and embodied in charts of the winds and currents, and in the compilation of sailing directions to every part of the globe. For some years the instructions thus furnished received very little attention, and very few observations were made or communicated; the publication, however, in 1848, of some charts, founded upon the discussion of the scanty materials which had come to hand or which could be collected from other sources, and which indicated much shorter routes than had hitherto been followed to Rio and other ports of South America, was sufficient to satisfy some of the more intelligent

shipmasters of the object and real importance of the scheme, and in less than two years from that time it had received the cordial cooperation of the masters of nearly every ship that sailed. At the present time there are nearly 1000 masters of ships who are engaged in making these observations; they receive freely in return the charts of the winds and currents, and the sailing directions which are formed upon them, corrected up to the latest period.

Short as is the time that this system has been in operation, the results to which it has led have proved of very great importance to the interests of navigation and commerce. The routes to many of the most frequented ports in different parts of the globe have been materially shortened, that to St. Francisco in California by nearly one third: a system of southwardly monsoons in the equatorial regions of the Atlantic and on the west coast of America has been discovered; a vibratory motion of the trade-wind zones, and with their belts of calms and their limits for every month of the year, has been determined: the course, bifurcations, limits and other phenomena of the Great Gulf-stream have been more accurately defined, and the existence of almost equally remarkable systems of currents in the Indian Ocean, on the coast of China, and on the North-western coast of America and elsewhere has been ascertained: there are, in fact, very few departments of the science of meteorology and hydrography which have not received very valuable additions; whilst the more accurate determination of the parts of the Pacific Ocean, where the sperm-whale is found (which are very limited in extent), as well as the limits of the range of those of other species, has contributed very materially to the success of the American whale fishery, one of the most extensive and productive of all the fields of enterprise and industry.

The success of this system of cooperative observations has already led to the establishment of societies at Bombay and Calcutta, for obtaining, by similar means, a better knowledge of the winds, currents, and the course of the streams of the Indian seas.

But it is to the government of this country that the demand for cooperation, and for the interchange of observations, is most earnestly addressed by the government of the United States; and the President and Council of the Royal Society express their hope that it will not be addressed in vain. We possess in our ships of war, in our packet service and in our vast commercial navy, better means of making such observations, and a greater interest in the results to which they lead, than any other nation; for this purpose, every ship which is under the control of the Admiralty should be furnished with instruments properly constructed and compared, and with proper instructions for using them: similar instructions for making and recording observations, as far as their means will allow, should be sent to every ship that sails, with a request that the results of them be transmitted to the Hydrographer's Office of the Admiralty, where an adequate staff of officers or others should be provided for their prompt examination, and the publication of the improved charts and sailing directions to which they would lead; above all, it seems desirable to establish a prompt communication

with the Hydrographer's Office of the United States, so that the united labours of the two greatest naval and commercial nations of the world may be combined, with the least practicable delay, in promoting the interests of navigation.

The President and Council refer to the documents which have been submitted to them, and more especially to the "Explanations and Sailing Directions to accompany wind and current charts" prepared by Lieutenant Maury, for a more detailed account of this system of cooperative observations, and of the grounds upon which they have ventured to make the preceding recommendations.

(Signed) S. HUNTER CHRISTIE, Sec. R.S.

H. U. Addington, Esq.

3. "Second Appendix to a paper entitled 'Discovery that the Veins of the Bat's Wing (which are furnished with valves) are endowed with rythmical contractility.'" By T. Wharton Jones, Esq., F.R.S. &c.

The author states that, from a microscopical examination of the blood-vessels and circulation in the ears of the long-eared bat, he has ascertained that, different from what he had discovered to be the case in the wings, the veins of the ears are unfurnished with valves, and are not endowed with rythmical contractility, and that the onward flow of blood in them is consequently uniform.

4. A paper was in part read, entitled, "Upon the Morphology of the Cephalous Mollusca, as illustrated by the anatomy of certain Heteropoda and Pteropoda." By Thomas Huxley, Esq., F.R.S. Received March 18, 1852.

May 27, 1852.

The EARL OF ROSSE, President, in the Chair.

The reading of Mr. Huxley's paper, "Upon the Morphology of the Cephalous Mollusca, as illustrated by the Anatomy of certain Heteropoda and Pteropoda," was resumed and concluded.

In the present memoir the author endeavours to determine, upon anatomical and embryological grounds, the true homologies of the different organs of the Cephalous Mollusca, and thence to arrive at some idea of the archetypal form, as definite modifications of which the existing molluscous forms may be considered to have arisen.

The Pelagic Heteropoda and Pteropoda, from their small size and extreme transparancy, are peculiarly favourable subjects for the anatomical part of this investigation, and it is from a detailed examination of those systems of organs which are of importance for the purpose that the author deduces the following conclusions:—

1. In the *Heteropoda* the intestine is bent towards the dorsal or *hæmal* side in consequence of the development behind the anus of the visceral "hernia," which is therefore called a *post-abdomen*.

2. In the *Heteropoda*, the "foot," in its most perfect condition, consists of three portions, a *propodium, mesopodium* and *metapodium.*

3. The *Heteropoda* are more or less prosobranchiate, the degree depending upon the amount of development of the post-abdomen.

4. In the *Pteropoda* the intestine is bent towards the ventral or *neural* side, in consequence of the development of the visceral "hernia" in front of the anus. It is therefore called an *abdomen.*

5. In the *Pteropoda,* the foot, besides the parts mentioned above, possesses an additional appendage, the *epipodium,* which forms the expanded wing characteristic of the group.

6. The *Pteropoda* are opisthobranchiate, prosobranchiate, or intermediate in character, according to the degree of development of the *abdomen.*

The *Heteropoda* and *Pteropoda,* then, may be considered to represent two opposite phases of the modification of the molluscous archetype.

In the second part of the paper, the author endeavours, by carefully collating the known facts of the development of the Mollusca, to ascertain (*a*) the primary form of all cephalous Mollusca, and (*b*) the mode in which, in the course of development, this embryonic form becomes metamorphosed into the adult form; in order, if possible, to account, on the safe basis of ascertained morphological laws, for the peculiar modifications of structure which have been found, anatomically, to obtain among the Heteropoda and Pteropoda.

He finds that it is possible not only to deduce the structure of the Heteropoda and Pteropoda from a simple and symmetrical archetype by such morphological laws, but that all the cephalous Mollusca fall under one or other of the great types of which these have been taken as exemplifications.

After a discussion of the various theories of the homology of the organs of cephalous Mollusca proposed by Loven, Leuckart, &c., the following general conclusions are set forth:—

1. The cephalous Mollusca are all organized after the same fundamental form or archetype.

2. The arrangement of the systems of organs within this archetype is essentially the same as in the Vertebrata and Annulosa; that is to say, supposing the digestive system to form the axis of the body, the nervous centre lies on one side of that axis; the blood-vascular centre upon the opposite; and furthermore, the archetype is symmetrical with regard to a longitudinal vertical plane, passing through these three.

3. The *molluscous* archetype differs from the *vertebrate* in the circumstance—1, that the mouth opens upon the neural surface; 2, that the embryo commences its development upon the hæmal side.

It differs from the *articulate* archetype in the latter circumstance, and from both in the fact, that the proper appendicular system (represented by the epipodium) is almost rudimentary, and that the locomotive function is mainly performed by a development of the neural surface.

4. The process of concentration and fusion of parts by which the

principal modifications are produced among the Vertebrata and Articulata, seems almost absent in the Mollusca; the changes among them being produced by an asymmetrical development of the primarily symmetrical archetype, a process comparatively rare among the Articulata and Vertebrata.

5. The part thus asymmetrically developed is invariably a portion of the hæmal surface, and may be called an *abdomen* or a *post-abdomen*, according as it is placed before or behind the anus.

6. The intestine is found to be bent in two directions among the Mollusca, hæmad or neurad, and these flexures correspond with the development of a post-abdomen or abdomen, respectively.

7. The process of development demonstrates that the Tectibranchiata, Nudibranchiata and Pectinibranchiata (in part at least) belong to the former division, and that the Cephalopoda and Pulmonata belong to the latter.

8. Anatomical evidence shows that the Heteropoda have a hæmal flexure of the intestine, the Pteropoda a neural flexure; and it is almost certain that when their development is traced, the former will be found to have a post-abdomen, the latter an abdomen; there will then be two great divisions of the cephalous mollusca.

a. Those which develope an abdomen:—*Cephalopoda, Pteropoda, Pulmonata.*

b. Those which develope a post-abdomen:—*Heteropoda, Pectinibranchiata, Tectibranchiata, Nudibranchiata.*

9. Prosobranchism and Opisthobranchism may occur as secondary results of either course of development.

10. The principal nervous centres are similar in number and position throughout, and differ only in their arrangement and degrees of concentration. The amount of the latter does not correspond with the complexity of organization of the mollusk, but rather the reverse.

11. The organization of the vascular system is equally uniform; its completeness or incompleteness is no mark of complexity or simplicity of the rest of the organization.

12. The cephalous Mollusca are characterized by the possession of a peculiar organ, the dentigerous "tongue," whose mode of action resembles that of a chain-saw.

13. The locomotive apparatus, when fully developed, consists of four parts, the propodium, mesopodium, metapodium and epipodium. These parts are least modified in such mollusks as Atlanta or Pneumodermon; most altered and disguised in such as Cleodora or Octopus.

14. The term "mantle" should be confined to the surface of the *abdomen* or *post-abdomen*, and to the prolonged edges of that surface.

15. It is of great importance to recollect that the "shells" are probably not homologous organs in all the different forms of mollusks.

The shells of Sepia, Spirula (?), Limax, Clausilia and Helix are developed in the thickness of the mantle.

The shells of Nautilus (?), Pectinibranchiata, &c., are developed from the surface of the mantle by a quite distinct process.

Certain curious differences appear to follow the externality or internality of the shell.

An external shell in a mollusk with a hæmal flexure, *e.g.* Atlanta, has its columellar axis *below* the aperture.

An external shell in a mollusk with a neural flexure, *e.g.* Nautilus, has its columellar axis *above* the aperture.

An internal shell in a mollusk with a neural flexure, has its columellar axis *below* the aperture, *e.g.* Spirula, Clausilia, Helix.

In the course of the memoir the author incidentally introduces a number of new, and, as he believes, important facts, with regard to the nervous, circulatory and urinary systems; and describes at length the mechanism of the "tongue" and an organ similar to the "crystalline style" of bivalves, found in the Strombidæ.

The following papers were also read:—

1. "On the Change of Refrangibility of Light." By George G. Stokes, Esq., M.A., F.R.S., Lucasian Professor of Mathematics, Cambridge. Received May 11, 1852.

The author was led into the researches detailed in this paper by considering a very singular phenomenon which Sir John Herschel had discovered in the case of a weak solution of sulphate of quinine, and various other salts of the same alkaloid. This fluid appears colourless and transparent, like water, when viewed by transmitted light, but exhibits in certain aspects a peculiar blue colour. Sir John Herschel found that when the fluid was illuminated by a beam of ordinary daylight, the blue light was produced only throughout a very thin stratum of fluid adjacent to the surface by which the light entered. It was unpolarized. It passed freely through many inches of the fluid. The incident beam, after having passed through the stratum from which the blue light came, was not sensibly enfeebled nor coloured, but yet it had lost the power of producing the usual blue colour when admitted into a solution of sulphate of quinine. A beam of light modified in this mysterious manner was called by Sir John Herschel *epipolized*.

Several years before Sir David Brewster had discovered in the case of an alcoholic solution of the green colouring matter of leaves a very remarkable phenomenon, which he has designated as *internal dispersion*. On admitting into this fluid a beam of sunlight condensed by a lens, he was surprised by finding the path of the rays within the fluid marked by a bright light of a blood-red colour, strangely contrasting with the beautiful green of the fluid itself when seen in moderate thickness. Sir David afterwards observed the same phenomenon in various vegetable solutions and essential oils, and in some solids. He conceived it to be due to coloured particles held in suspension. But there was one circumstance attending the phenomenon which seemed very difficult of explanation on such a supposition, namely, that the whole or a great part of the dispersed beam was unpolarized, whereas a beam reflected from suspended

particles might be expected to be polarized by reflexion. And such was, in fact, the case with those beams which were plainly due to nothing but particles held in suspension. From the general identity of the circumstances attending the two phenomena, Sir David Brewster was led to conclude that epipolic was merely a particular case of internal dispersion, peculiar only in this respect, that the rays capable of dispersion were dispersed with unusual rapidity. But what rays they were which were capable of affecting a solution of sulphate of quinine, why the active rays were so quickly used up, while the dispersed rays which they produced passed freely through the fluid, why the transmitted light when subjected to prismatic analysis showed no deficiencies in those regions to which, with respect to refrangibility, the dispersed rays chiefly belonged, were questions to which the answers appeared to be involved in as much mystery as ever.

After having repeated some of the experiments of Sir David Brewster and Sir John Herschel, the author could not fail to take a most lively interest in the phenomenon. The firm conviction which he felt that two portions of light were not distinguishable as to their nature otherwise than by refrangibility and state of polarization, left him but few hypotheses to choose between, respecting the explanation of the phenomenon. In fact, having regarded it at first as an axiom that dispersed light of any particular refrangibility could only have arisen from light of the same refrangibility contained in the incident beam, he was led by necessity to adopt hypotheses of so artificial a character as to render them wholly improbable. He was thus compelled to adopt the other alternative, namely, to suppose that in the process of internal dispersion the refrangibility of light had been changed. Startling as such a supposition might appear at first sight, the ease with which it accounted for the whole phenomenon was such as already to produce a strong probability of its truth. Accordingly the author determined to put this hypothesis to the test of experiment.

The experiments soon placed the fact of a change of refrangibility beyond all doubt. It would exceed the limits of an abstract like the present to describe the various experiments. It will be sufficient to mention some of the more remarkable results.

A pure spectrum from sunlight having been formed in air in the usual manner, a glass vessel containing a weak solution of sulphate of quinine was placed in it. The rays belonging to the greater part of the visible spectrum passed freely through the fluid, just as if it had been water, being merely reflected here and there from motes. But from a point about half-way between the fixed lines G and H to far beyond the extreme violet the incident rays gave rise to light of a sky-blue colour, which emanated in all directions from the portion of the fluid which was under the influence of the incident rays. The anterior surface of the blue space coincided of course with the inner surface of the vessel in which the fluid was contained. The posterior surface marked the distance to which the incident rays

were able to penetrate before they were absorbed. This distance was at first considerable, greater than the diameter of the vessel, but it decreased with great rapidity as the refrangibility of the incident rays increased, so that from a little beyond the extreme violet to the end the blue space was reduced to an excessively thin stratum adjacent to the surface by which the incident rays entered. It appears therefore that this fluid, which is so transparent with respect to nearly the whole of the visible rays, is of an inky blackness with respect to the invisible rays more refrangible than the extreme violet. The fixed lines belonging to the violet and the invisible region beyond were beautifully represented by dark planes interrupting the blue space. When the eye was properly placed, these planes were of course projected into lines. The author has made a sketch of these fixed lines, which accompanies the paper. They may be readily identified with the fixed lines represented in M. Becquerel's map of the fixed lines of the chemical spectrum. The last line seen in a solution of sulphate of quinine appears to be the line next beyond the last represented in M. Becquerel's map. Under very favourable circumstances two dusky bands were seen still further on. Several circumstances led the author to conclude that in all probability fixed lines might be readily seen corresponding to still more refrangible rays, were it not for the opacity of glass with respect to those rays of very high refrangibility.

It is very easy to prove experimentally that the blue dispersed light corresponding to any particular part of the incident spectrum is not homogeneous light, having a refrangibility equal to that of the incident rays, and rendered visible in consequence of its complete isolation; but that it is in fact heterogeneous light, consisting of rays extending over a wide range of refrangibility, and not passing beyond the limits of refrangibility of the spectrum visible under ordinary circumstances. To show this it is sufficient to isolate a part of the incident spectrum, and view the narrow beam of dispersed light which it produces through a prism held to the eye.

In Sir David Brewster's mode of observation, the beam of light, which was of the same nature as the blue light exhibited by a solution of sulphate of quinine, was necessarily mixed with the beam due merely to reflexion from suspended particles; and in the case of vegetable solutions, a beam of the latter kind almost always exists, to a greater or less degree. But in the method of observation employed by the author, to which he was led by the discovery of the change of refrangibility, the two beams are exhibited quite distinct from one another. The author proposes to call the two kinds of internal dispersion just mentioned *true internal dispersion* and *false internal dispersion*, the latter being nothing more than the scattering of light which is produced by suspended particles, and having, as is now perfectly plain, nothing to do with the remarkable phenomenon of true internal dispersion.

Now that the nature of the latter phenomenon is better known, it is of course possible to employ methods of observation by which it may be detected even when only feebly exhibited. It proves to be

almost universal in vegetable solutions, that is, in solutions made directly from various parts of vegetables. When vegetable products are obtained in a state of isolation, their solutions sometimes exhibit the phenomenon and sometimes do not, or at least exhibit it so feebly that it is impossible to say whether what they do show may not be due to some impurity. Among fluids which exhibit the phenomenon in a high degree, or according to the author's expression are highly *sensitive*, may be mentioned a weak decoction of the bark of the horse-chestnut, an alcoholic extract from the seeds of the *Datura stramonium*, weak tincture of turmeric, and a decoction of madder in a solution of alum. In these cases the general character of the dispersion resembles that exhibited by a solution of sulphate of quinine, but the tint of the dispersed light, and the part of the spectrum at which the dispersion begins, are different in different cases. In the last fluid, for example, the dispersion commences somewhere about the fixed line D, and continues from thence onwards far beyond the extreme violet. The dispersed light is yellow, or yellowish orange.

In the case of other fluids, however, some of them sensitive in a very high degree, the mode in which light is dispersed internally presents some very remarkable peculiarities. One of the most singular examples occurs in the case of an alcoholic solution of the green colouring matter of leaves. This fluid disperses a rich red light. The dispersion commences abruptly about the fixed line B, and continues from thence onwards throughout the visible spectrum and a little beyond. The dispersion is subject to fluctuations intimately connected with the singular absorption bands exhibited by this medium.

In order that a medium should be capable of changing the refrangibility of light incident upon it, it is not necessary that the medium should be a fluid, or a clear solid. Washed papers and other opake substances produce the same effect, but of course the mode of observation must be changed. The author has observed the change of refrangibility in various ways. It will be sufficient to mention here that which was found most generally useful, which he calls the method of observing by a *linear spectrum*. The method is as follows.

A series of prisms and a lens are arranged in the usual manner for forming a pure spectrum, but the slit by which the light enters, instead of being parallel, is placed in a direction perpendicular to the edges of the prisms. A linear spectrum is thus formed at the focus of the lens, consisting of an infinite succession of images of the slit arranged one after the other in the order of refrangibility, and of course overlapping each other to a certain extent. The substance to be examined is placed in the linear spectrum, and the line of light seen upon it is viewed through a prism held to the eye. In this way it is found that almost all common organic substances, such as wood, cork, paper, calico, bone, ivory, horn, wool, quills, feathers, leather, the skin of the hand, the nails, are sensitive in a greater or less degree. Organic substances which are dark-coloured are fre-

quently found to be insensible, but, on the other hand, scarlet cloth and various other dyed articles are highly sensitive. By means of a linear spectrum the peculiar dispersion of a red light produced by chlorophyll, or some of its modifications, may be observed not only in a solution, but in a green leaf, or on a washed paper, or in a sea-weed.

The highly sensitive papers obtained by washing paper with tincture of turmeric, or a solution of sulphate of quinine, or some other highly sensitive medium, display their sensibility in a remarkable manner when they are examined in a linear spectrum. In these cases, however, the paper produces a very striking effect when merely held so as to receive a pure spectrum formed in the usual manner, that is, with a slit parallel to the edges of the prisms. Such a paper may be used as a screen for showing the fixed lines belonging to the invisible rays, though they are not thus shown quite so well as by using a solution. The extraordinary prolongation of the spectrum seen when it is received on turmeric paper, has been already observed by Sir John Herschel, by whom it was attributed to a peculiarity in the reflecting power of that substance. Of course it now appears that the true explanation is very different.

A high degree of sensibility appears to be rather rare among inorganic compounds. Certain specimens of fluor spar, as is already known, give a copious internal dispersion of a deep blue light; but this is plainly due to some foreign ingredient, the nature of which is at present unknown. But there is one class of inorganic compounds which are very remarkable for their sensibility, namely, certain compounds of peroxide of uranium, including the ornamental glass called canary glass, and the natural mineral yellow uranite. In these compounds the dispersed light is found on analysis to consist of bright bands arranged at regular intervals. A very remarkable system of absorption bands is also found among these compounds, which is plainly connected with the system of bright bands seen in the spectrum of the dispersed light. The connection between the absorption and internal dispersion exhibited by these compounds is very singular, and is of a totally different nature from the connection which has been already mentioned as occurring in solutions of the green colouring matter of leaves.

There is one law relating to the change of refrangibility which appears to be quite universal, namely, that the refrangibility of light is *always lowered* by internal dispersion. The incident rays being homogeneous, the dispersed light is found to be more or less composite. Its colour depends simply on its refrangibility, having no relation to the colour of the incident light, or to the circumstance that the incident rays were visible or invisible. The dispersed light appears to emanate in all directions, as if the solid or fluid were self-luminous while under the influence of the incident rays.

The phenomenon of the change of refrangibility of light admits of several important applications. In the first place it enables us to determine instantaneously the transparency or opacity of a solid or fluid with respect to the invisible rays more refrangible than the

violet, and that, not only for these rays as a whole, but for the rays of each refrangibility in particular. For this purpose it is sufficient to form a pure spectrum with sun-light as usual, employing instead of a screen a vessel containing a decoction of the bark of the horse-chestnut, or a slab of canary glass, or some other highly sensitive medium, and then to interpose the medium to be examined, which, if fluid, would have to be contained in a vessel with parallel sides of glass. Glass itself ceases to be transparent about the region corresponding to the end of the author's map, and to carry on these experiments with respect to invisible rays of still higher refrangibility would require the substitution of quartz for glass. The reflecting power of a surface with respect to the invisible rays may be examined in a similar manner.

The effect produced on sensitive media leads to interesting information respecting the nature of various flames. Thus, for example, it appears that the feeble flame of alcohol is extremely brilliant with regard to invisible rays of very high refrangibility. The flame of hydrogen appears to abound in invisible rays of still higher refrangibility.

By means of the phenomena relating to the change of refrangibility, the independent existence of one or more sensitive substances may frequently be observed in a mixture of various compounds. In this way the phenomenon seems likely to prove of value in the separation of organic compounds. The phenomena sometimes also afford curious evidence of chemical combinations; but this subject cannot here be further dwelt upon.

The appearance which the rays from an electric spark produce in a solution of sulphate of quinine, shows that the spark is very rich in invisible rays of excessively high refrangibility, such as would plainly put them far beyond the limits of the maps which have hitherto been made of the fixed lines in the chemical part of the solar spectrum. These rays are stopped by glass, but transmitted through quartz. These circumstances render it probable that the phosphorogenic rays of an electric spark are nothing more than rays of the same nature as those of light, but which are invisible, and not only so, but of excessively high refrangibility. If so, they ought to be stopped by a very small quantity of a substance known to absorb those rays with great energy. Accordingly the author found that while the rays from an electric spark, which excite the phosphorescence of Canton's phosphorus, pass freely through water and quartz, they are stopped on adding to the water an excessively small quantity of sulphate of quinine.

At the end of the paper the author explains what he conceives to be the cause of the change of refrangibility, and enters into some speculations to account for the law according to which the refrangibility of light is always lowered in the process of internal dispersion.

2. "Analytical Researches connected with Steiner's Extension of Malfatti's Problem." By Arthur Cayley, M.A., Fellow of Trinity College, Cambridge. Communicated by J. J. Sylvester, Esq., F.R.S. Received April 12, 1852.

The problem, in a triangle to describe three circles each of them touching the two others and also two sides of the triangle, has been termed after the Italian geometer, by whom it was proposed and solved, Malfatti's Problem. The problem to which the author refers as Steiner's extension of Malfatti's Problem, is as follows: "To determine three sections of a surface of the second order, each of them touching the two others, and also two of three given sections of the surface of the second order," a problem proposed in Steiner's memoir 'Einige Geometrische Betrachtungen,' Crelle, t. i. The geometrical construction of the problem in question is readily deduced from that given in the memoir just mentioned for a somewhat less general problem, viz. that in which the surface of the second order is replaced by a sphere; it is for the sake of the analytical developments to which the problem gives rise that the author proposes to resume here the discussion of the problem. The following is an analysis of the present memoir:—

§ 1. Contains a lemma which appears to the author to constitute the foundation of the analytical theory of the sections of a surface of the second order.

§ 2. Contains a statement of the geometrical construction of Steiner's extension of Malfatti's problem.

§ 3. Is a verification, founded on a particular choice of co-ordinates, of the construction in question.

§ 4. In this section, referring the surface of the second order to absolutely general co-ordinates, and after an incidental solution of the problem to determine a section touching three given sections, the author obtains the equations for the solution of Steiner's extension of Malfatti's problem.

§ 5. Contains a separate discussion of a system of equations, including as a particular case the equations obtained in the preceding section.

§§ 6 & 7. Contain the application of the formulæ for the general system to the equations in § 4, and the development and completion of the solution.

§ 8. Is an extension of some preceding formulæ to quadratic functions of any number of variables.

3. "On the Tides, Bed and Coasts of the North Sea or German Ocean." By John Murray, Esq. Communicated by George Rennie, Esq., F.R.S. Received March 20, 1852.

The author commences his paper by remarking that great similarity of outline pervades the western shores of Ireland, Scotland and Norway, and then observes that the great Atlantic flood-tide wave, having traversed the shores of the former countries, strikes with great fury the Norwegian coast between the Lafoden Isles and Stadland, one portion proceeding to the north, while the other is deflected to the south, which last has scooped out along the coast, as far as the Sleeve at the mouth of the Baltic, a long channel from 100 to 200 fathoms in depth, almost close in shore, and varying from 50 to 100 miles in width. After describing his method of con-

touring and colouring the Admiralty chart of the North Sea, he traces the course of the tide-wave among the Orkney and Shetland Islands along the eastern shores of Scotland and England to the Straits of Dover, and along the western shores of Norway, Denmark and the Netherlands, to the same point. He then remarks that the detritus arising from the continued wasting away of nearly the whole line of the eastern coasts of Scotland and England, caused by the action of the flood-tide, is carried by it, and at the present day finds a resting-place in the North Sea; and that this filling process is increased by the sand, shingle, and other matter brought through the Straits of Dover by the other branch of the Atlantic flood-tide. Hence, he remarks, the gradual shoaling of this sea, and the formation of its numerous sand-banks; the silting up the mouths of the Rhine, the Meuse, and the Scheldt; the formation of the numerous islands on the coast of Holland, that country itself, and much of Belgium; the deposits at the mouth of the Baltic, the islands in the Cattegat, and indeed the whole country of Sleswig, Denmark and Jutland..

The author then takes a view of the tides, and their effects upon the Baltic and its shores before the course of the tide-wave was checked by these shoals and low lands. He considers that, previous to these great changes, the flood-tide entering the North Sea between Norway and Scotland, would make directly towards the German coast, and necessarily heap up the waters in the Baltic considerably above their present level, and that a great part of Finland, Russia, and Prussia bordering upon that sea, would thus every twelve hours be under water, in the same way as the waters now rise in the Bay of Fundy, at Chepstow, and other places, much above their ordinary level in the open sea; that the current outward, on the receding of the tide which these accumulated waters would occasion, combined with the rivers which fall into the Baltic, when checked by the following flood-tide, would cause deposits in the form of a bar tailing towards Sweden; and that an increase to these deposits would form shoals, drifts and islands, and eventually a long sand-bank in outline, like the country of Denmark. He further considers that the tide being by these means prevented from entering the Baltic, may account for the subsidence of the waters of the Gulf of Bothnia better than can the upheaval of the northern part of Scandinavia.

The author then remarks that the great shoal of the North Sea is the Dogger Bank, and that its peculiar form is produced by the meeting of the cotidal waves, of which he traces the course. After bearing testimony to the value of the Admiralty chart of the southern portion of the North Sea, made under the direction of the late Captain Hewitt, he reverts to the importance of contouring such maps, in order to obtain something like a correct notion of the bottom of the sea; and in conclusion expresses a hope that the Admiralty will be induced to continue the survey of the North Sea, so well begun by Captain Hewitt.

June 3, 1852.

The EARL OF ROSSE, President, in the Chair.

The Annual Meeting for the Election of Fellows was held this day.

The Statutes for the Election of Fellows having been read, Dr. Forbes and Dr. McWilliam were, with the consent of the Society, appointed Scrutators.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected:—

Arthur Kett Barclay, Esq.	Hugh Lee Pattinson, Esq.
Rev. Jonathan Cape.	Rev. B. Price.
Arthur Cayley, Esq.	William Simms, Esq.
Henry Gray, Esq.	Hugh E. Strickland, Esq.
Wyndham Harding, Esq.	John Tyndall, Esq.
Arthur Henfrey, Esq.	Nathaniel Bagshaw Ward, Esq.
John Higginbottom, Esq.	Captain Younghusband, R.A.
John Mercer, Esq.	

The Society then adjourned to the 10th of June.

June 10, 1852.

The EARL OF ROSSE, President, in the Chair.

The following gentlemen were admitted into the Society:—

Arthur Kett Barclay, Esq.	Hugh Lee Pattinson, Esq.
Arthur Cayley, Esq.	William Simms, Esq.
Henry Gray, Esq.	Hugh E. Strickland, Esq.
Arthur Henfrey, Esq.	William Thomson, Esq.
John Higginbottom, Esq.	Captain Younghusband, R.A.

The following papers were read:—

1. "On the Structure and Development of Bone." By John Tomes, F.R.S., Surgeon Dentist to the Middlesex Hospital, and Campbell De Morgan, Surgeon to the Middlesex Hospital. Received April 22, 1852.

In this communication, the authors, after having briefly noticed the intimate structure of perfect bone as commonly recognised, proceed to the description of certain points connected with its structure and development, which they believe to have been hitherto entirely overlooked or only partially recognised.

These points have been arranged under the following heads:—

1. The Haversian and other canals of bone.
2. The laminæ of bone.
3. The lacunæ.
4. Haversian systems.
5. Ossified cartilage of joints.

6. Ossified cells.
7. Bone tissue.
8. Development of bone in temporary cartilage.
9. Growth of bone.

1. *Haversian and other canals of bone.*—Besides the Haversian canals, the authors have pointed out that there are found in bone sections spaces of an entirely different character, irregular in shape, and with an irregular festooned margin. Their margins correspond in outline with those of one or more Haversian systems, and precede in many instances the formation of those systems. These spaces produced by absorption are called by the authors *Haversian spaces.* Unlike the Haversian canals which are surrounded by their own laminæ, these spaces are bounded by parts of several systems which have been encroached on by the process of absorption.

In examining various sections, or different parts of the same section, these spaces will be found in different states of partial or entire occupation by Haversian systems. They are found in the bone of subjects of all ages. The fact of removal of old tissue and replacement by new, which has been hitherto only assumed, is thus demonstrated.

2. *Laminæ of bone.*—Lamination is shown to be a constant character of mammalian bone; each lamina, when highly developed, is found to consist of a dark granular, and of a transparent part. The external margin of the outermost lamina of each Haversian system is irregularly indented and corresponds with the outline of a pre-existing Haversian space; while its internal margin and all the succeeding laminæ are regular in outline.

The laminæ are found as a general rule to surround their canal, which is usually placed in the centre of them. But sometimes the canals are eccentric, in which case either the laminæ on one side, though still surrounding the canal, are broader, or there are more developed on one side than on the other. The lamina next to the perfected Haversian canal however is always complete, and is often composed of a transparent structureless tissue, like that which encircles the Haversian canals of the stag's antler at the time of shedding.

The presence of interstitial laminæ is readily accounted for; they are in fact the remains of pre-existing Haversian systems, or circumferential laminæ, parts of which have been removed by absorption.

The circumferential laminæ are not so constantly present as is generally described, and rarely entirely surround the shaft of a long bone. When present, they seem to indicate that the bone is nearly stationary in its growth. They are frequently intersected by numerous Haversian spaces and systems, so as at length to assume the characters of interstitial laminæ.

3. *Lacunæ.*—In young bone the lacunæ are more abundant, larger, and have more numerous canaliculi; or they may exist without canaliculi, or the canaliculi and great part of the lacunæ themselves may be filled up with solid matter, so as to leave only a small space in the centre of the latter. The lacuna and canaliculi are shown to have distinct walls.

In the circumferential laminæ are frequently found elongated tubes which the authors regard as modifications of lacunæ; they run obliquely across the laminæ, generally in bundles. They frequently form communications with the canaliculi. In transverse section they are seen to have proper walls.

4. *Haversian systems.*—The authors have here pointed out that the anastomosis of the canaliculi of adjoining systems is rare in newly-developed systems, but is very common in those of greater age. It has been seen too that it not unfrequently happens that a series of Haversian systems is contained within a common series of surrounding laminæ. Sometimes the Haversian systems are rendered quite solid by the narrowing of the Haversian canal and ultimate development of a mere lacuna in the centre of the system. The more recently developed Haversian systems which occupy Haversian spaces are seen to be darker in colour than the older ones, from the greater abundance of canaliculi, and the more general granularity of the tissue.

5. *Ossified articular cartilage.*—This structure the authors have found in all the joints which they have examined, in the lower jaw, amongst others, where Kölliker failed to detect it. Towards the bone the tissue becomes in general granular and of a brownish colour, and usually there is a distinct line of demarcation between the bone and the ossified cartilage; but sometimes they graduate insensibly the one into the other.

Towards the articular surface the margin is even and regular; but towards the bone it is deeply indented, from the bone advancing into it by rounded projections. Hence the articular cartilage varies in thickness. The authors believe that this, so far from being an indication of imperfect development, is in reality an evidence of design, and intended to give an uniform and unyielding surface for the cartilage to rest upon.

6. *Ossified cells.*—In the bones of aged people it is frequently observed that they become light and spongy, and after maceration contain a white powder in the cancellated structure. This powder the authors have found to be composed mainly of ossified nucleated cells, either detached or held together in masses. They are spherical, and contain a dark granular nucleus, which is surrounded by a thick transparent wall.

If portions of the cancelli be taken, they will be found to have similar cells adherent to their surfaces, or to those of the Haversian canals, with here and there canaliculi of adjoining lacunæ shooting into them, while the nuclei have themselves assumed the form of lacunæ. Similar cells may be found imbedded in parts of most sections of bone. In order to see this condition clearly, it is desirable that the sections and the loose cells should be mounted in Canada balsam.

7. *Bone tissue.*—The views generally entertained with regard to the ultimate structure of bone tissue are, the older one, that it consists of an aggregation of granules in a transparent matrix; and that

which has been more recently put forward by Dr. Sharpey, that in many cases it is composed of ossified decussating fibres.

The authors have satisfied themselves that the ultimate structure of bone tissue is composed of minute granules or granular bodies imbedded in a clear or subgranular matrix; and that the appearance of fibres is due in many cases to the mode of illumination. By transmitted light passing through them in the long axis of the microscope the preparations show a granular or a structureless appearance, or alternations of a granular and structureless part. But under an oblique light passing from one side only an appearance of minute flat fibres presents itself. This takes place even in the isolated cells of old bone, or in developing young bone. This appearance is most marked over the lacunæ and canaliculi. But if a part which thus appears fibrous be viewed under a light passing obliquely from all sides, as is effected by a Gillett's achromatic condenser, the fibres disappear, and we see only a granular appearance, with some tendency to arrangement in the granules. The fibrous appearance is in fact due to the shadows cast from the less transparent parts when the light passes obliquely, just as in the navicula the dots are replaced by lines. In thin sections torn from bone which has been macerated in acid, a reticulated appearance, similar to that figured by Dr. Sharpey, may be seen, only however when the object is slightly out of focus, or the light oblique and from one side. By careful adjustment of the object-glass and of the illuminating apparatus, this appearance may be shown to depend on the presence of the canaliculi.

8 and 9. *Development and growth of bone.*—The early condition of cartilage, and the changes which take place in it and in the cartilage cell before ossification, are particularly described; and also the mode by which they multiply and arrange themselves by segmentation, so that a long column or cluster of cells represents an original cell, the walls of which have coalesced with the surrounding hyaline tissue. The cells at the same time enlarge individually as they approach the point where ossification is going on, encroaching on the hyaline substance so as in many cases only to leave a fine line of intercolumnar tissue, or to cause it to disappear altogether. The nucleus at the same time enlarges considerably, while the cell wall becomes thickened internally, until in the end it reaches the nucleus, which then becomes imbedded in firm tissue. Other changes now take place: either several cells are thrown into one cavity by the absorption of their contiguous walls, leaving the nuclei free in the common cavity; or the nucleus continues to occupy its parent cell, and sends off small processes, which extend outwards to the cell wall. At this stage the nucleus may be sometimes detached with the processes entire, but generally it is adherent, and may be seen to have become a lacuna with a central cavity and canaliculi; in addition to which a nucleus may be seen to occupy its interior; it has in fact become a nucleated cell, designated by the authors "*granular cell.*" The entire cell may now be detached from the intercolumnar tissue in which it lies.

The granular condition of the intercolumnar tissue and of the cell itself often renders the observation of this stage very difficult; but in rickety bone it is very readily shown, as in this disease there is a tendency for the cells to assume their permanent form before the deposit of bone-earth in any considerable quantity. To cells thus composed of an outer thickened cell wall and an inner granular cell (the cartilage nucleus of authors) which contains within it a nucleus (the nucleolus of writers), which stands in the relation of a nucleus to the future lacuna, the authors have given the name of "*lacunal cells*," while the term granular cell has been applied to that which is usually designated the nucleus. In transverse sections of bone immediately below the line of ossification, the lacunal cells may be seen presenting different characters under different circumstances. Where two cells come into contact, the processes or canaliculi may be seen extending across from one to the other; but where the cell is surrounded by intercolumnar tissue, the processes are short and do not extend beyond the walls of their own cell; or if cells join at one point while the remainder is inverted with intercolumnar tissue, the canaliculi will anastomose at the point of junction; while elsewhere they are few, short, and do not extend beyond the cell.

In the further process of development the cells and intercolumnar tissue become fused together so as no longer to be recognised as distinct parts; and the granular cell appears as a perfect lacuna with a large cavity and numerous large canaliculi. To bone in this condition the term primary bone has been applied. It speedily however undergoes a change preparatory to the formation of the more permanent secondary bone. Here and there in the line of ossification portions are removed by absorption, the spaces left being filled with small somewhat granular cells lying in a transparent blastema, through the agency of which the absorption has been in all probability effected. It would appear as though the cells grew at the expense of the surrounding tissue. These spaces correspond entirely to the Haversian spaces before described; and in them the secondary bone is in the first instance formed. The process of formation of secondary bone appears to be everywhere essentially the same, whether in the absorbed spaces, or on the surfaces, or in the membranes of the fœtal cranium, except that in the two latter cases there is a pre-existing fibrous tissue, which, before ossification begins, undergoes a change similar to that which occurs in the bone itself and is converted into a cellular mass. So that at the border where ossification is advancing there is only an arrangement of cells; while a little beyond that point the cells have fibrous tissue abundantly mixed up with them; and there is in fact a resemblance to fibrous tissue in an early state of formation. The formation of perfect bone is effected by means of cells, perhaps identical with those which are found replacing the previous tissue, but at all events undistinguishable from them by any microscopical characters. To these cells, which take part in the formation of bone, the authors have given the name of "*osteal cells*." In the case of laminated bone they arrange themselves side by side, and, together with the transparent blastema in

which they lie, become impregnated with ossific matter, and permanently fused with the bone tissue with which they lie in contact. By the linear arrangement of these osteal cells lamination is produced. In the case of new laminated bone the cells are simply ossified without arrangement. Lying amongst the osteal cells will be seen some which have accumulated around them a quantity of tissue which forms a thick investment to them; they then become granular, and take on in every respect the characters of a lacunal cell. These are found deposited at intervals along the line of ossification and becoming blended with the general mass; the granular cell remaining as a lacuna, and sending out processes amongst the cells in all directions. In old bone the cell character is in great part lost by a general blending of the constituents, but may in many specimens be still here and there recognised. Many instances are given in support of the conclusion that absorption of bone and of dental tissue is effected directly through the influence of cells, but these are necessarily excluded from this abstract; indeed it is impossible to give any other than a very imperfect account of the contents of the paper within the prescribed limits, especially as the numerous illustrations which accompany the paper cannot be made use of.

2. "On Rubian and its Products of Decomposition. Part II. Action of Alkalies and Alkaline Earths on Rubian." By Edward Schunck, Esq., F.R.S. Received April 19, 1852.

From the author's experiments it appears that rubian is decomposed by the fixed alkalies, and by lime and baryta, but not by ammonia. The products of decomposition formed by the action of the alkalies and alkaline earths are five in number. They are as follows:—1st, *Alizarine*; 2nd, *Verantine*; 3rd, *Rubiretine*; 4th, *Sugar*; and 5th, a new substance, which the author denominates *Rubiadine*. The first four possess the same properties and composition as when formed by the action of acids on rubian. The fifth substance, rubiadine, occupies the place of rubianine, which it closely resembles. It crystallizes from an alcoholic solution in small yellow or orange-coloured needles. It is insoluble in boiling water, and when carefully heated it may be almost entirely volatilized, forming a sublimate of yellow micaceous scales, endowed with considerable lustre. By these two properties it may be distinguished from rubianine, which is soluble in boiling water, and cannot be heated without being decomposed. Its other properties coincide almost entirely with those of rubianine. Its composition is expressed by the formula $C_{32}\,H_{12}\,O_8$, and presuming that the formula for rubianine be $C_{32}\,H_{19}\,O_{15}$, it would differ from the latter only by the elements of 7 equivs. of water. Besides these substances, there is also formed a small quantity of a dark brown powder, which is soluble in alkalies, but insoluble in water and alcohol. This substance has precisely the same composition as the ulmic acid of Mulder, formed by the action of strong acids on cane-sugar. Its formation is doubtless due to the further action of the alkali on the sugar formed in the first instance.

Action of Ferments on Rubian.—It has long been suspected by

chemists, that the colouring matter of madder owes its formation to some process of fermentation, but the exact nature of the process has hitherto remained unknown. That some process of decomposition takes place on extracting madder with cold tepid water and exposing the extract to a moderate temperature, is proved by the fact that the extract, if concentrated, becomes after some time thick and gelatinous; and that the process of decomposition takes effect chiefly on the rubian is apparent, since the extract, after it has become gelatinous, is found to have lost its bitter taste and the greater part of its yellow colour.

In order to prepare the peculiar fermentative substance of madder, which has the power of effecting the decomposition of rubian, it is merely necessary to add to an extract of madder made with cold or tepid water, about an equal volume of alcohol. This causes the separation of a quantity of dark reddish-brown flocks, which are collected on a filter and washed with cold alcohol, until the percolating liquid, which is at first strongly coloured, becomes almost colourless. The substance on the filter has the appearance of a dark reddish-brown granular mass, which possesses in an eminent degree the power of decomposing rubian. It is a true ferment, to which the author gives the name of *Erythrozym*. If a quantity of it be added to a solution of rubian, and the mixture be left to stand at the ordinary temperature, a complete change is found to have taken place in the course of a few hours. The liquid is converted into a trembling jelly of a light brown colour, which is perfectly tasteless, insoluble in cold water, and so consistent, that if the solution of rubian was tolerably concentrated, the vessel may be turned over without its falling out. During this process none of the usual signs of fermentation are manifested. The liquid remains perfectly neutral, and no gas of any kind is disengaged. On treating the gelatinous mass resulting from the process with cold water, an almost colourless liquid is obtained, which contains the same species of sugar as that formed by the action of acids or alkalies on rubian. The mass left undissolved by the cold water consists partly of the ferment employed and partly of the substances formed by the decomposition of the rubian. These substances are six in number, of which three are bodies previously described, and three are new. They are,—1st, *Alizarine*; 2nd, *Verantine*; 3rd, *Rubiretine*; 4th, a substance closely resembling rubiacine, which the author calls *Rubiafine*; 5th, a substance very similar to rubianine and rubiadine, on which he bestows the name of *Rubiagine*; and 6th, a peculiar fatty substance which he denominates *Rubiadipine*.

The three latter bodies, which are products peculiar to this process of decomposition, have the following properties:—

Rubiafine is obtained by crystallization from alcohol in yellow glittering plates and needles, which are sometimes arranged in star-shaped or plume-shaped masses. It cannot be distinguished by any of its properties from rubiacine; its composition is however different. Like the latter it is converted by the action of persalts of iron into rubiacic acid. The author has again submitted rubiacine and rubi-

acic acid, together with some of their compounds, to analysis, and from a comparison of their composition with that of rubiafine, he infers that the formula of the latter is $C_{32} H_{13} O_9$, that of rubiacine $C_{32} H_{11} O_{10}$, that of rubiacic acid $C_{32} H_9 O_{17}$; and he inclines to the belief, that by the action of persalts of iron rubiafine first passes into rubiacine, before being converted into rubiacic acid; while by the action of reducing agents rubiacic acid is reconverted; first into rubiacine, and then into rubiafine.

Rubiagine is never obtained in well-defined crystals. When its alcoholic solution is evaporated spontaneously, it is left behind in the shape of small lemon-yellow spherical grains, which when crushed and examined under a lens, are found to consist of small crystalline needles grouped round a centre. When heated it melts, and is decomposed without being volatilized. It is quite insoluble in boiling water. It is soluble in boiling nitric acid, with a yellow colour, and crystallizes out again on the solution cooling in shining needles. Its alkaline solutions are blood-red. The alcoholic solution gives on the addition of acetate of lead at first no precipitate, but the colour of the solution becomes dark yellow, and after some time, provided the solution be not too dilute, an orange-coloured granular precipitate subsides, which is the lead compound of rubiagine. If no deposit is formed, then the addition of water causes an orange-coloured flocculent precipitate, which after being washed with water, in order to remove the excess of acetate of lead, is found to be very little soluble in boiling alcohol, but is easily soluble in a boiling alcoholic solution of acetate of lead with a dark yellow or orange-colour. When rubiagine is treated with a boiling solution of perchloride of iron, it dissolves slightly, but is not converted into rubiacic acid. Rubiagine is distinguished from rubianine by its insolubility in water; from rubiadine, for which it might most easily be mistaken, by its being incapable of sublimation; and from rubiafine by its not being convertible into rubiacic acid. Its behaviour towards acetate of lead, which is different from that of all the other three substances, also serves to characterize it. The most probable formula for rubiagine is $C_{32} H_{14} O_{10}$, from which it appears that it differs from the substances just named merely by the elements of water. Its formation from rubian, like that of the substances allied to it, indicates the simultaneous formation of sugar.

Rubiadifine is a body of a fatty nature, as its name indicates. In its appearance and general properties it resembles rubiretine. It differs from the latter in always remaining soft and viscid, and never becoming hard and brittle, however long it may be heated. Its colour is yellowish-brown. When heated in a tube it emits acrid fumes, similar to those produced by fat when exposed to destructive distillation. When thrown into boiling water it melts and forms oily drops, which rise to the surface. Its alcoholic solution gives with acetate of lead a pale reddish-brown precipitate, soluble in an excess of the precipitant. An analysis of the lead compound conducted to the formula $C_{30} H_{24} O_5 + PbO$. If this formula be accepted as the true one, the author confesses his inability to explain the formation

of rubiadifine from rubian. The great excess of hydrogen contained in it shows that some substance must be formed simultaneously containing a large proportion of oxygen, which has hitherto escaped detection.

Having examined generally the action of erythrozym on rubian, the author next proposed to himself to inquire, by what means this action is either destroyed, retarded, or promoted, and whether any means exist of so modifying the action as to lead to the formation of particular substances in preference to others. From a variety of experiments undertaken with this object, he draws the following conclusions:—

1. There exist no means short of the complete destruction of the ferment, capable of arresting its action on rubian, except exposing it while in a moist state to the temperature of boiling water. Even when exposed to that temperature after having been previously dried, its fermenting power is not entirely lost, but merely weakened.

2. By the addition of various substances, usually classed as antiseptic, such as sulphuric acid, arsenious acid, sugar of lead, corrosive sublimate, alcohol, and oil of turpentine, during the process of fermentation, the action of the ferment is not destroyed; it is merely retarded and modified.

3. The more the action of the ferment on rubian is retarded, the more rubiretine and verantine, and the less alizarine are formed; so much so, that in some cases the alizarine disappears entirely from among the products of decomposition, which then consist almost solely of rubiretine and verantine. The formation of rubiafine and rubiagine is promoted when the action of the ferment is moderately retarded, but diminishes again or entirely ceases when the retardation is very great. Of the two the rubiagine is the first to disappear when any retardation takes place.

4. By the addition of small quantities of alkalies during the process of fermentation, the action is, as regards its duration, if not promoted, at all events not retarded; and as regards the relative quantities of the various substances produced, the amount of alizarine is thereby decidedly increased, while that of the rubiretine and verantine is diminished.

Most of the ordinary fermentative substances, such as albumen, caseine, gelatine, and yeast, are incapable of effecting the decomposition of rubian, even when mixtures of these various substances with watery solutions of rubian are allowed to stand until they enter into a state of putrefaction. Emulsine is the only substance capable of forming an effective substitute for erythrozym. Its action is similar to that of the latter substance; it gives rise however to the formation of a much larger proportional quantity of alizarine. The action of the peculiar albuminous substance, discovered by Braconnot in the root of the *Helianthus tuberosus*, on rubian, was also examined. It exerts only a slight effect on the latter, the only products of decomposition formed being rubiretine and verantine.

The author considers the fact of erythrozym being almost the only

ferment which is capable of effecting in any considerable degree the decomposition of rubian, as the best proof of its being a peculiar and distinct substance. When prepared by precipitation with alcohol, erythrozym is obtained as a chocolate-coloured granular mass. When dried it coheres into hard lumps, which are almost black, and with difficulty reduced to powder. When the dry substance is heated on platinum foil, it emits a smell somewhere between that of burning peat and burning horn, and then burns without much flame, leaving a considerable quantity of residue, which on being further heated is soon converted into a white or grey ash.

After having once been precipitated from its watery solution, even by alcohol, erythrozym cannot again be dissolved in water. If it be mixed while in a moist state with water, it forms a reddish-brown turbid liquid, in which it exists however merely in a state of suspension. Erythrozym is not an uncombined substance, but is a definite compound of an organic body with lime. When treated with acids the lime is removed, and the colour of the substance changes from reddish-brown to yellowish-brown. If a mixture of erythrozym and water be allowed to stand for some time, the former enters into a state of putrefaction, accompanied by a disengagement of gas. After it has entered on this stage of decomposition, it loses in great part the power of decomposing rubian, but acquires that of producing an acid reaction in a solution of sugar. Erythrozym contains nitrogen, but in much smaller proportion than most other fermentative substances. Its composition, when in a freshly precipitated state, is expressed by the formula $C_{56}H_{34}N_2O_{40}+4CaO$. When allowed to decompose, it loses carbonic acid, water, and lime. A quantity which had been employed for the purpose of effecting the decomposition of rubian, and then separated again from the products of the action, had a composition corresponding to the formula $C_{52}H_{32}N_2O_{30}+3CaO$.

In conclusion, the author gives the results of some experiments undertaken with the view of ascertaining whether madder contains more than one colouring matter or not. He infers from his experiments, that the purpurine of other chemists is not a substance of determinate composition; that it consists sometimes of alizarine alone, sometimes of verantine alone, sometimes of a variable mixture of both; that only one colouring matter, viz. alizarine, can be obtained from madder; that purpurine, madder-purple, and the various similar bodies derived from madder, owe their property as colouring matters to an admixture of alizarine; and that they are simply the latter substance in a state of impurity.

3. "Experiments towards the construction of new forms of Instruments for the correction of Compass Errors due to the presence of iron in ships; with investigations on the nature of the attraction of Iron on the poles of Magnets." By Julius Roberts, Esq., Lieut. R.M. Artillery. Communicated by Capt. W. H. Smyth, R.N., For. Sec. R.S. Received March 25, 1852.

The object of the author's experiments and investigations is stated

to have been the production of an instrument that would, under all variety of circumstances, give a correct (magnetic) meridian direction, or in some way indicate the amount of its own error; and he considers that he has, at least partially, succeeded in the attainment of this object in the instrument described in this paper, and of which drawings accompany the communication.

In order to determine the nature of the action of a mass of iron on a magnetic needle, the author constructed needles with the magnetic bar wholly on one side of the central support, counterpoised by an arm carrying a weight on the other. In some of these the magnetic bar was straight, in others it was bent in the middle, either the upper or lower half being horizontal. He also constructed a compound needle of two such bent bars, suspended concentrically with two of their contrary poles remote, the other two poles being the one above and the other below the points of support. In order to give magnetic stability to the compound needle, an arc of soft iron was attached to the extremity of the counterpoising arm of each magnet, so as to be in close proximity, though not actually to touch the other. Attached to the counterpoise arms were the halves of a compass-card cut through the east and west points, and so adjusted that the edge of one was vertically over that of the other when the needles were in the same vertical plane, but crossed each other when the needles deviated from the same direction. The author considered that a mass of iron attracting each pole of this compound needle would cause the cards to cross each other, and thus give the amount of deviation due to that mass, but found that, instead of the cards crossing, the needles deviated in contrary directions so as to remain in one straight line, as if they had been rigidly connected.

To obviate this, for the magnet having its unmarked end pointing south was substituted one having its marked end in that direction, but of less power than that of which the marked end pointed north; and two magnets rigidly connected in the same straight line, with their marked ends remote, were balanced concentric with the other two. The author considers that the result of this combination would be, that this astatic bar would, by the repulsion between its poles and those of the other magnets, take up a position at right angles to the magnetic meridian, the other magnets pointing due north and south. Further, that the north pointing and south pointing ends of the compound needle on the half cards being poles of the same name, that is, both marked, any mass of iron which attracted one end would also attract the other, and thus the half cards which they carried would be caused to cross each other. So that the whole being properly adjusted, the deviations caused by the iron on the two needles will be equal, and the error of deviation of either one will be half the angle between the two. A figure of this complicated compass accompanies the description.

In conclusion, the author states that the practicability of rendering the instrument so simple and accurate as to be generally useful, depends on experiments and investigations yet to be made; and

then enters upon a calculation, from which, however, no definite results are deduced.

June 17, 1852.

The EARL OF ROSSE, President, in the Chair.

The following gentlemen were admitted:—

Rev. Jonathan Cape.	John Tyndall, Esq.
John Mercer, Esq.	

The following gentlemen were recommended by the Council for election as Foreign Members;—

Adolphe Theodore Brongniart.	J. Lamont.
Benjamin Peirce.	V. Regnault.

1. "On the Impregnation of the Ovum in the Amphibia (Second Series revised), and on the direct agency of the Spermatozoon." By George Newport, F.R.S., F.L.S. &c. Received June 17, 1852.

The author remarks that, having in a former paper shown that the spermatozoon alone is the impregnatory agent, he endeavoured in a subsequent communication to the Royal Society, a report of which is printed in the Proceedings for June 1851, to arrive at some conclusion as to the nature of its influence; and, from the facts he was then acquainted with, he announced the view that the spermatozoon appeared to be the organ of a special form or condition of force in the animal body. At that time he had no evidence that the spermatozoon penetrates into the coverings of the egg, as he had constantly found it attached only to the surface. Since then he has detected it within the substance of these coverings, and sometimes even partially imbedded in the vitelline membrane beneath them; but he has no evidence that it enters the vitelline cavity. While therefore the fact of penetration into the envelopes necessitates some revision of the details of the view announced, he still regards the spermatozoon as the organ of a special condition or form of force in the animal structure.

He then proceeds to show the relative duration of vitality in the spermatozoon and the egg, and points out that that of the former is shorter than is usually supposed; that at the temperature of 55° Fahr. it usually is lost in from three to four hours after removal from the body into water; but that at a lower temperature it is retained longer, and that when the spermatic fluid has contained many undeveloped cells, and has been preserved in a temperature of 51° Fahr., it has fertilized at the end of twenty-four hours. The egg loses its fitness to be impregnated very soon after it is passed into water, usually within the first hour, owing chiefly to the endosmosis and expansion of its envelopes. But when retained within the body of

the dead frog its vitality is preserved for twenty-four, and sometimes even for forty-eight hours, at a low temperature. He next shows that the results produced by the active vibratile spermatozoa on the dead egg are similar to those which are at first produced on the living one by solution of potass, viz. the yelk becomes shrivelled and contracted, and this result also occurs when decomposing spermatic fluid is applied to it. Having repeated the experiments formerly mentioned (Proceedings, p. 83), that the frog's egg may be fecundated by application of exceeding minute quantities of spermatozoa by means of the head, and even of the point of a small pin, to almost any part of its surface, he shows that there are some parts of the surface more, and some less, susceptible than others; and that, in a series of careful experiments made with a view to test this fact, he found that when the egg is placed vertically, with the centre of the white surface uppermost, and the spermatozoa are applied to this part, and not allowed to flow over the sides of the egg, fecundation is then but rarely effected; but that when the centre of the dark surface is uppermost, and the spermatozoon is applied to that part, fecundation of the egg is then almost invariably the result. A fact is also mentioned which is of some value in experiments on artificial impregnation. The chamber which is formed above the yelk in the fecundated egg, as described in the author's former paper, is commenced at the end of the first hour, by the contraction and depression of the upper or dark surface of the yelk; and thus affords an early proof as to whether or not the egg has been fecundated. When no chamber is formed, it is certain that the egg has not been fecundated. But the chamber may be formed, and the yelk not undergo segmentation; in which case fecundation has been only partial and incomplete. The motion of the spermatozoon in relation to its function is then examined, and the author states that he regards this motion as only the visible exponent of a peculiar power in the impregnating agent, and as essential to its function, and that it is associated with the material composition and structure of this body, the degree of procreative efficiency of which, he thinks, is indicated by the degree, or intensity of its motive power; although he believes that some portion of the substance of the body of the spermatozoon is also communicated to the egg in fecundation.

The author then shows that having adopted a mode of examining the egg, beneath the microscope, at the time of the spermatozoa being supplied to it, different from that which he formerly employed, he has been enabled to detect the fact of penetration by the spermatozoon into the envelopes, and its arrival at the vitelline membrane, with great facility. Availing himself of the fact previously ascertained, that impregnation may be effected by the direct application of the spermatozoon by means of the pin's head or point, he put the fact of penetration to a very positive test beneath the microscope, and found that the spermatozoon always penetrates at the parts to which it is applied, and at no other part of the egg, and that a short time afterwards it may be detected striking into the vitelline membrane, by its thicker or body portion, in a line with the point at

which it has entered, and the centre of the yelk, and that he has usually found eggs so penetrated to have been fertilized and produce embryos. Further, that eggs in which no spermatozoa have been seen in contact with the yelk membrane, have usually been unfruitful, although numerous spermatozoa have been observed on their surface. During his experiments the author had an opportunity of examining some eggs which had been impregnated by the natural concurrence of the sexes, and then found that these most fully confirmed the results obtained by artificial impregnation. Spermatozoa were observed sticking into the vitelline membrane for many hours after the time at which the egg must have been fecundated; which the author believes must be within the *first half-hour*, and perhaps within the *first few minutes*; as he has sometimes found spermatozoa close to the vitelline membrane *within one minute* after they had been supplied to the egg. The spermatozoon invariably enters the egg with its thicker or body portion foremost, and passes onwards with a direct but slightly serpentine motion, in a centripetal direction to the vitelline membrane. A large proportion of the spermatozoa never enter the envelopes of the egg, if they happen to come into contact with them laterally, as is frequently the case; they then merely adhere to the surface, but do not fecundate. The greatest number penetrate when supplied to the egg within a few seconds after removal from the male body, and of the eggs from the body of the female. Some experiments are then detailed which the author states arose out of a communication made to him by Mr. Busk, F.R.S. The spermatozoa were narcotized by exposure for eight or ten minutes to the vapour of chloroform, and it was then found that when in this state, while perfectly motionless, as when dead, they did not impregnate. These experiments, in connection with others, seemed to show that the entrance of the spermatozoon into the egg is not the result of an endosmic action of the envelopes, but is that of the operation of a distinct power in the spermatozoon.

The nature of the influence of the spermatozoon is then examined. The author has endeavoured to put this to the test of experiment, *first*, by immersion of eggs, both before, and at the period of fecundation, and during the segmentation of the yelk, in solutions of potass; and *next*, by reducing the bodies of recently-obtained and perfectly active spermatozoa, to a fluid state, by trituration in a glass mortar, prepared for the purpose, and then applying the materials so obtained to the egg immediately it is expelled from the female, and before it has been in contact with water; and consequently at the time it is most susceptible of the fecundatory influence. The experiments by immersion in potass solution showed that the endosmic action of the egg envelopes is exceedingly rapid, as decomposition of the yelk was commenced in some within *three* minutes of the application of the solution. In very weak solution the result was different, and appeared to be favourable to the action of the spermatozoon. The fluid obtained by trituration of the spermatozoa was applied to several sets of eggs, but no fecundation of the egg was effected by it; the yelks, however, became affected, being in some

cases shrivelled and contracted, as when potass solution or decomposing seminal fluid is applied, thus showing that the substance of the broken down spermatozoa had passed to the yelk by endosmosis. These experiments were made at the same time with others made with portions of the same fluid which had not been triturated, and in which the spermatozoa were still active. In these instances fecundation was constantly effected, so that the conclusion deduced from these comparative trials was, that fecundation is not the result simply of the addition of the substance of the body of the spermatozoon to the egg, but primarily seems to be due to a force or dynamic power in the spermatozoon, which is lost when this body has ceased to give evidence of its retention of it, in its power of motion.

The author then proceeds to inquire whether these results do not justify our viewing the spermatozoon as the organ of a special form or condition of force in the animal structure? and states, as he has done on a former occasion (Proceedings, June 1851, p. 83), other grounds on which the hypothesis seems to be supported, pointing out that the spermatozoon, like muscle and nerve, has both general and special anatomical structure and special chemical composition; and that as we have been accustomed to regard the power of muscular contractility as a distinct force, or form of force, of the body,—the same view being held with regard to nerve, the properties of these two tissues being perfectly distinct from each other,—so it appears to be correct to view the property of the spermatic structure; which is not only perfectly distinct from either of these, but different from that of every other tissue in the organization, and is not exercised until the structure itself has been entirely separated from the body of which it originally formed a part.

2. "On the Functions of the Membrana Tympani, the Ossicles and Muscles of the Tympanum, and of the Eustachian Tube in the Human Ear, with an account of the Muscles of the Eustachian Tube and their action in different classes of Animals." By Joseph Toynbee, Esq., F.R.S. &c. Received June 15, 1852.

The author commences his paper by making some observations on the general arrangements of the *ossicula auditûs*. The malleus and incus being firmly connected together by ligaments, are considered as a single bone, forming an elastic arch, the anterior extremity of which is firmly attached to the Glasserian fissure, the posterior to the anterior part of the mastoid cells. This arch is kept steady by the actions of the tensor tympani. The movement of this arch is that of rotation; and it is effected by the tensor tympani muscle. When this muscle contracts, the lower part of the arch, consisting of the handle of the malleus and the long process of the incus, is drawn inwards; by this action the membrana tympani is rendered tense, and the stapes being pressed towards the cavity of the labyrinth, the fluid in the latter is compressed.

The anatomy and attachments of the *stapes* are next minutely described. The base of this bone, generally stated by writers on

the physiology of the ear as being attached to the margin of the *fenestra ovalis* by a simple membrane, *ligamentum annulare baseos stapidis*, is shown to possess some points of considerable interest. Instead of a simple margin to which the ligament above noticed is fixed, the base of the stapes is stated to present a circumferential surface for articulation with the *fenestra ovalis*. This circumferential surface, larger at the anterior and posterior extremities than in the middle, is covered by articular cartilage. The surface of the *fenestra ovalis*, to which the circumference of the base of the stapes is applied, is somewhat larger than that of the stapes. It is smooth, has a very compact structure, and is not covered by cartilage.

The base of the stapes is attached to the *fenestra ovalis* by two ligaments. The inner or vestibular ligament passes from the inner margin of the *fenestra ovalis* to the inner margin of the circumferential surface of the base of the stapes. The outer one passes from the outer margin of the *fenestra ovalis* to the corresponding margin of the stapes. These two circular ligaments leave between them a space which may be considered as an articular cavity; this cavity always containing a sufficient quantity of fluid to lubricate the articular surfaces of the bones.

The movements of the stapes are of two kinds, one being produced by the action of the *tensor tympani*, the other by the *stapedius* muscle. If the *tensor tympani* muscle be drawn in the direction of its course, while the cavity of the vestibule has been exposed to view, the base of the stapes will be observed to be slightly projected towards the cavity of the vestibule, and it returns to its normal position as soon as the muscle is left quiescent: the ligaments above described appear to be the organs whereby the stapes is again drawn outwards. In this movement the stapes may therefore be described as passing to and fro within the *fenestra ovalis*, as a piston does in a cylinder. The *second* movement of which the base of the stapes partakes is one of rotation, and it is effected by the stapedius muscle. To show this movement, it is requisite to perform one or two careful experiments. The stapedius muscle being exposed in its canal, while the stapes is left undisturbed in the tympanic cavity which has been laid open, if the muscle be drawn in the direction of its course, the anterior crus of the stapes is observed to move slightly outwards and backwards. In what manner the base of the stapes is affected during this movement of the crura it is difficult to decide, but it would appear probable that its anterior part is drawn outwards from the cavity of the vestibule while the posterior part is pressed inwards, though to a less extent. That the action of the stapedius muscle is to relax the fluid of the labyrinth, is however shown by the following experiment. The tympanic cavity and stapedius muscle being exposed to view, a section is to be made through the cochlea, a small portion of the *scala vestibuli* being left continuous with the cavity of the vestibule. If the stapedius muscle be now drawn in the direction of its course, the fluid in the cut extremity of the *scala vestibuli* is observed to recede slightly towards the vestibule, and it returns to its former position

as soon as the stapedius muscle is left quiet. A second action of the *stapedius* muscle is to act as a laxator of the *membrana tympani*, and it thus appears that the *stapedius* muscle is the antagonist of the *tensor tympani*; and it seems to be brought into action during the act of listening, while the *tensor tympani*, on the contrary, contracts when the ear has to be protected from any loud vibrations.

The next part of the paper is devoted to the consideration of the functions of the *membrana tympani*; which, besides the one usually ascribed to it, viz. of receiving the sonorous vibrations from the air and of conducting them to the chain of bones and thence to the labyrinth, the author considers to be as follows:—

1. To act in conjunction with the ossicles and muscles of the tympanum as the analogue of the iris in the eye, and to exclude from reception by the labyrinth, of such strong vibrations as would be injurious to its integrity; also, in exactly opposite circumstances, to receive the most faint undulations, which would not be perceived unless the *membrana tympani* were rendered less tense than is the case in ordinary circumstances. The former of the two duties is performed by the *tensor tympani* muscle, the latter by the *stapedius*. The *membrana tympani* is not only of use in preventing powerful sonorous vibrations from compressing too forcibly the expansion of the auditory nerve, but it also protects the labyrinth from the forcible pressure of air or of a foreign substance in the meatus, during a blow on the ear or the introduction of a solid body which presses against the *membrana tympani*.

2. The *membrana tympani* shuts out the air in the meatus from that in the tympanic cavity, and by this means an atmosphere of certain physical conditions is constantly kept in contact with the membranes between the tympanum and labyrinth.

The *second* part of the paper is devoted to the examination of the Eustachian tube in man and animals. Anatomists seem to have inferred that the Eustachian tubes in their natural state are constantly open, and that the air of the tympanic cavities is always continuous with that in the fauces. An examination of the guttural portion of the Eustachian tube in man and animals has led the author to conclude, that excepting during muscular effort, this orifice is always closed, and that the tympanum is a cavity distinct from the outer air. The agents whereby the Eustachian tubes are opened in the human subject, are the muscles of the palate, and it is by their action during the process of swallowing that the tubes are ordinarily opened. That the act of swallowing is the means whereby the Eustachian tubes are opened, is shown by some experiments of which the following may be cited. If the mouth and nose be closed during the act of swallowing, a sensation of fulness and distension is produced in the ears; this arises from the air, which is slightly compressed in the fauces, passing into and filling the tympanic cavities; upon removing the hand from the nose, it will be observed that this feeling of distension does not disappear, but remains until the act of deglutition is again performed, but while the nose is not closed; in this experiment the Eustachian tubes were opened during each act

of swallowing; but during the first act, while they were open, air was forced into the cavity of the tympanum by the contraction of the muscles of the fauces and pharynx, and the orifices were again closed, and remained so until the second act of swallowing, which opened the tube and allowed the air to escape. That the act of deglutition opens the Eustachian tubes, was inferred also from the custom usually adopted of swallowing while the descent in a diving-bell is performed; by this act the condensed air is allowed to enter the tympanum and the sensation of pain and pressure in the ears is avoided. The author then proceeds to show that the *tensor* and *levator palati* are the muscles which are attached to and open the Eustachian tubes in man, and the mode in which they act is pointed out.

The *third* part of the paper is devoted to the examination of the Eustachian tube in animals; and the author arrives at the conclusion, that in Mammalia, Birds, and those reptiles having a tympanic cavity, the Eustachian tubes, as in man, are closed excepting during muscular effort. In some *mammalia* the muscles opening the tubes are, as in man, those belonging to the palate; in others the function is performed by the superior constrictor of the pharynx. In *birds* it is shown that there is a single membranous tube into which the two osseous tubes open; this membranous tube is situated between, and intimately adherent to, the inner surface of each pterygoid muscle, and by which muscles the tube is opened.

The conclusion respecting the influence of the closed Eustachian tubes to which the author arrives, is that the function of hearing is best carried on while the tympanum is a closed cavity; that the analogy usually cited as existing between the ordinary musical instrument, the drum and the tympanum, to the effect that in each it is requisite for the air within to communicate freely with the outer air, is not correct. The view that the sonorous vibrations of the air in a closed tympanic cavity are more effective in impressing the membrane of the *fenestra rotunda* than when it is open to the outer air, is strengthened by the performance of the following experiment with the tuning-fork. If this instrument be made to vibrate by striking it against a firm solid, and if the handle be then placed in contact with the head, the sound at first loud, gradually becomes fainter, and soon ceases to be heard; if at the moment that it has ceased to be heard, a finger be placed over the tragus of one ear, and firmly pressed so as to close the external meatus from the outer air, the sound of the tuning-fork is again heard, and continues to be heard for some seconds; thus showing that the sonorous undulations existing in the external meatus are not sufficiently powerful to affect sensibly the *membrana tympani* until they are wholly confined by the walls of the tube when closed.

The leading results arrived at in the paper are as follows:—

1. That a principal function of the *membrana tympani* muscles and ossicles of the tympanum, is to act as the analogue of the iris in the eye.

2. That the *tensor tympani* muscle, by drawing tense the *membrana tympani*, and by compressing at the same time the fluid in the

labyrinth, protects the ear from the injurious influence of very powerful vibrations.

3. The stapedius muscle, by slightly relaxing the *membrana tympani* and the fluid of the labyrinth, places the ear in a position to be influenced by vibrations of a most delicate character.

4. Another function of the *membrana tympani* is to form part of the resonant walls of the closed tympanic cavity.

5. The guttural orifices of the Eustachian tubes are closed, and the tympanic cavities do not communicate with the cavity of the fauces excepting during certain muscular actions.

6. In man and some mammalia the Eustachian tubes are opened by the muscles of the palate, in other animals by the superior constrictor of the pharynx.

7. In birds there is a membranous tube common to the two osseous Eustachian tubes, and this common tube is opened by the action of the internal pterygoid muscles.

8. For the function of hearing to be perfect, it is requisite that the tympanic cavity should be closed from the outer air.

3. " An Experimental Inquiry undertaken with the view of ascertaining whether any, and what signs of current Force are manifested during the organic process of Secretion in living animals " (continued). By H. F. Baxter, Esq. Communicated by R. B. Todd, M. D., F.R.S. &c. Received April 30, 1852.

The present communication is a continuation of a series of experiments, the first part of which was published in the Phil. Trans. for the year 1848. The object is to show that the changes which occur during the organic process of secretion in living animals are accompanied with the manifestation of current force; and the principal facts upon which this conclusion is founded are the following:—

1st. It was found that when the electrodes of a galvanometer are brought into contact with the secreted product and the venous blood flowing from the same organ, an effect upon the needle is produced, indicating the venous blood to be *positive*. This fact was established in the liver, kidneys and mammary gland.

2ndly. The effect could not be referred to the heterogeneity of the fluids without assuming that the blood was *acid* and *combined* with the secreted product; nor could it be referred entirely to *thermo-electric* effects, inasmuch as the current varied in each organ, and was capable of traversing a liquid conductor. The effects, however, may be partly due to *catalytic actions on the combining power of platinum*; and this last supposition tended to confirm the opinion originally entertained by Wollaston, that the changes which occur during secretion are analogous to those which take place in the *decomposing* cell of a voltaic circle.

Without giving any definite opinion as to the lungs performing the office of a secreting organ, it was found, that when one electrode was in contact with the mucous surface, and the other in contact with the blood in the pulmonary veins, an effect occurred upon the needle indicating the blood (arterial) to be *positive*. This fact ap-

pears to afford some explanation of the failures of Muller, Pouillet and of the author in his early attempts to obtain evidence of current force being manifested when a circuit was formed between an artery and a vein in the living animal.

4. "On a new Series of Organic Bodies containing Metals." By Dr. E. Frankland, Professor of Chemistry, Owen's College, Manchester. Communicated by B. C. Brodie, Esq., F.R.S. Received May 10, 1852.

The author communicates in this memoir the continuation of his researches, a preliminary announcement of which appeared several years ago, upon a new series of organic compounds closely allied to cacodyl in their composition and properties, and which, like that body, are formed by the union of the alcohol radicals with various metals, and are distinguished for their powerful electro-positive character. These remarkable compounds are procured by the action of heat or light upon their proximate constituents, and are thus distinguished from most other organic compounds of this nature by the manner of their formation. The author describes seven of these compounds.

Stanethylium.—When iodide of ethyl and metallic tin are exposed to the influence of heat or light, which is most conveniently done in sealed glass tubes, the tin gradually dissolves in the ethereal liquid, which finally solidifies to a mass of colourless crystals. A quantity of gas, comparatively very small, is generated at the same time. This gaseous product of the reaction proved, on analysis, to be a mixture of hydride of ethyl and olefiant gas, produced from the decomposition of iodide of ethyl by tin into iodide of tin and ethyl, which last is transformed at the moment of its liberation into the two gases just mentioned. The principal and most important reaction, however, consists in the direct union of tin with iodide of ethyl, giving rise to a crystalline body which is the iodide of a new organic radical, *stanethylium.*

By double decomposition the other compounds of stanethylium can be readily formed; the author has prepared and described the oxide, chloride, bromide and sulphide of stanethylium; these bodies exhibit a striking resemblance to the corresponding bi-compounds of tin, but are distinguished from them by a peculiar pungent and irritating odour resembling that of the volatile oil of mustard.

If a slip of zinc be immersed in a solution of chloride of stanethylium, dense oily drops soon form on the surface of the zinc, and finally collect at the bottom of the vessel. This oily liquid is the radical stanethylium, which possesses the following properties:—it exists at ordinary temperatures as a thick heavy oily liquid of a yellow or brownish-yellow colour, and an exceedingly pungent odour resembling that of its compounds, but much more powerful. It is insoluble in water, but soluble in alcohol and ether. At about 150° C. it enters into ebullition, but is simultaneously decomposed, metallic tin being deposited. In contact with air stanethylium rapidly absorbs oxygen, and is converted into oxide of stanethylium.

Chloride, iodide and bromide of stanethylium are instantaneously formed by the action of chlorine, iodine and bromine, or their hydrogen acids respectively, upon stanethylium. The two first are in every respect identical with the salts above mentioned, and the identity of the bromide is further proved by an ultimate analysis. The formula of stanethylium is $C_4 H_5 Sn$; that of the oxide $C_4 H_5 SnO$, and that of the bromide $C_4 H_5 Sn Br$. Stanethylium therefore perfectly resembles cacodyl in its reactions, combining directly with the electro-negative elements, and regenerating the compounds from which it has been derived.

Stanmethylium and stanamylium are formed when the iodides of methyl and amyl respectively are exposed to the action of light in contact with tin; their salts are isomorphous with those of stanethylium, but they have not yet been completely investigated.

Zincmethylium.—This radical is formed in an uncombined state when iodide of methyl and zinc are exposed to a temperature of about 150° C. in a sealed tube; the zinc gradually dissolves with an evolution of gas, whilst a mass of white crystals and a colourless mobile liquid refracting light strongly, occupy, after a few hours, the place of the original materials. In this reaction two distinct decompositions take place, viz. the decomposition of iodide of methyl by zinc with the production of iodide of zinc and liquid zincmethylium, and the decomposition of iodide of methyl by zinc with the formation of iodide of zinc and the gaseous radical methyl. The zincmethylium was obtained pure by distillation in an atmosphere of dry hydrogen. Its formula is $C_2 H_3 Zn$, and it possesses the following properties. It is a colourless, transparent and very mobile liquid, possessing a peculiar penetrating and insupportable odour, and boiling at a low temperature. Zincmethylium combines directly with oxygen, chlorine, iodine, &c., forming somewhat unstable compounds. Its affinity for oxygen is even more intense than that of potassium; in contact with atmospheric air it instantaneously ignites, burning with a beautiful greenish blue flame, and forming white clouds of oxide of zinc; in contact with pure oxygen it burns with explosion, and the presence of a small quantity of its vapour in combustible gases gives them the property of spontaneous inflammability in oxygen. Thrown into water, zincmethylium decomposes that liquid with the evolution of heat and light; when this action is moderated, the sole products of the decomposition are oxide of zinc and hydride of methyl.

The extraordinary affinity of zincmethylium for oxygen, its peculiar composition, and the facility with which it can be procured, cannot fail to cause its employment for a great variety of transformations in organic compounds; by its agency there is every probability that oxygen, chlorine, &c. can be replaced atom for atom by methyl, and thus entirely new series of organic compounds will be produced, and clearer views of the rational constitution of others be obtained.

The gaseous methyl formed simultaneously with zincmethylium is identical in composition and properties with the methyl derived from the electrolysis of acetic acid; it was mixed, however, with

hydride of methyl generated by the decomposition of accompanying zincmethylium vapour by the water over which the gas was collected.

Zincethylium and zincamylium are homologous bodies formed by similar processes; their investigation is not yet completed.

Hydrargyromethylium.—The author has only yet studied the iodide of this radical, which is formed by the action of sunlight upon iodide of methyl and metallic mercury. After an exposure of several days to sunlight, white crystals begin to form in the liquid, which finally solidifies to a white crystalline mass; ether dissolves out the new compound and deposits it perfectly pure by spontaneous evaporation.

Iodide of hydrargyromethylium ($C_2 H_3 HgI$) is a white solid, crystallizing in minute nacreous scales, which are insoluble in water, moderately soluble in alcohol, and very soluble in ether and iodide of methyl; it is slightly volatile at ordinary temperatures, and exhales a weak but peculiarly unpleasant odour, which leaves a nauseous taste upon the palate for several days. At 100° C. the volatility is much greater, and the crystals are rapidly dissipated at this temperature when exposed to a current of air. At 143° C. it fuses and sublimes without decomposition, condensing in brilliant and extremely thin crystalline plates. In contact with the fixed alkalies and ammonia it is converted into oxide of hydrargyromethylium, which is dissolved by an excess of all these reagents.

A corresponding compound containing amyl is formed, though with difficulty, under similar circumstances, but the attempts to form one containing ethyl have not yet been successful. Preliminary experiments have also been made with other metals, amongst which arsenic, antimony, chromium, iron, manganese and cadmium promise interesting results.

From a review of the composition and habits of all the organo-metallic bodies and their compounds at present known, the author is of opinion that the view most generally held respecting the constitution of cacodyl, according to which that radical is a conjugate compound consisting of arsenic conjugated with two atoms of methyl, and which view must, if true, be applied to all the organo-metallic bodies, is no longer tenable; and he contends that the behaviour of these bodies clearly indicates that they are compounds formed upon the type of the oxides of the respective metals, a portion of the oxygen being replaced by the several radicals, methyl, ethyl and amyl; the establishment of this new view of their constitution will remove these bodies from the class of organic radicals, and place them in the most intimate relation with ammonia and the bases of Wurtz, Hofmann and Paul Thenard; indeed the close analogy between stibethine and ammonia first suggested by Gerhardt, has been most satisfactorily demonstrated by the behaviour of stibethine with the haloid compounds of methyl and ethyl. Stibethine furnishes us therefore with a remarkable example of the law of symmetrical combination, and shows that the formation of a five-atom group from one containing three atoms can be effected by the assimilation of two atoms, either of the same or of opposite electro-chemical character: this

remarkable circumstance suggests the following question. Is this behaviour common also to the corresponding compounds of arsenic, phosphorus and nitrogen, and can the position of each of the five atoms with which these elements respectively combine be occupied indifferently by an electro-negative or an electro-positive element? This question, so important for the advance of our knowledge of the organic bases and their congeners, cannot now long remain unanswered.

5. "On the Dentate Body of the Cerebellum." By William Brinton, M.D. Communicated by R. B. Todd, M.D., F.R.S. &c. Received May 23, 1852.

The corpus dentatum has generally been described and recognised as a wavy line or lamina of grey matter, which is seen in certain sections of the crus of the cerebellum, and contains fibres apparently derived from the restiform body, and the processus e cerebello ad testes. Reil's account, with some vague and conflicting details, gives it a more definitely tubular form, although he is apparently not certain of the continuity of its upper and lower layers posteriorly.

The author explains these somewhat varying descriptions by the physical characters of the tissues investigated, and by the condition —fresh or hardened in spirit—of the specimens examined by different anatomists.

He deduces the form and situation of the recent corpus dentatum by uniting numerous and successive sections made in the three directions of space*. Its arrangement with respect to the fibres of the cerebellum, cerebrum, medulla oblongata, and medulla spinalis, is chiefly deduced from examinations of specimens hardened in alcohol.

By these two methods he is led to the following conclusions, that each corpus dentatum forms a tubular investment to the extremity of the processus e cerebello ad testem; it is open towards the fourth ventricle, and is connected with the opposite body by a commissure of grey matter in its median line. While its interior exclusively receives the fibres of this cerebro-cerebellar peduncle, its exterior radiates fibres to the various lobes of the cerebellum, which fibres, at the bottom of each lobe-stem, become inseparably mixed with a bundle from the restiform body, and with another from the pons varolii.

Its *comparative anatomy* in mammalia corresponds with this view; its *minute anatomy* does not contradict it. And while the *physiological import* of this arrangement eludes all conjecture, the author has little doubt that its anatomical structure and relations are best comprehended in the formula which he would thus assign to it, viz. that of being the cerebro-cerebellar ganglion.

6. "Proof of a sensible difference between the Mercurial and Air-Thermometers from 0° to 100° C." By J. J. Waterston, Esq.

* Diagrams to this effect accompanied the paper.

Communicated by Colonel Sabine, R.A., Treas. V.P.R.S. &c. Received June 17, 1852.

This paper has reference to a former communication "On a General Law of Density in Saturated Vapours." In the present paper the author states that the formulæ that embrace MM. Dulong and Petit's four standard mean values of the relative expansion of air, mercury and glass, exhibit the temperature by the air-thermometer in advance of the mercurial thermometer, between 0° and 100° C. The amount of difference increases from 0° to 48°, and then diminishes to 100°; the maximum value being 0°·513. The most eminent modern authorities deny the existence of any such difference, or appear tacitly to admit that it is too small to be observed. For this reason no correction was made on temperatures below 100° in Chart No. 2, where indeed it could hardly be perceptible. Although of little practical importance, this difference, if it exists, cannot safely be neglected in theoretical researches, inasmuch as the value of a degree of the mercurial thermometer must in such case be a variable quantity, differing in the ratio of 23 to 24 from 100° C. to 0°.

Having at last obtained satisfactory proof of the existence and amount of the correction between 0° and 100°, he has thought it of sufficient importance to give a detailed account of the method employed to extract the required evidence from M. Regnault's observations on the tension of low pressure steam.

As the law of density, illustrated in Chart No. 2, has clearly reference to the air-thermometer, if a series of observations were *perfectly* correct, they must *perfectly* exhibit this difference—if it really exists—when projected on the chart; because the divergence from the line that joins the points at 0° and 100° must exactly correspond with the correction required at the intermediate temperatures. In short, the line of density would appear as a curve slightly concave towards the axis, and if the proper correction were made on the temperatures, that curve would be converted into a straight line. This view is illustrated by a sketch, in which the curvature is purposely much exaggerated. In this a straight line is drawn, as the gradient of density, and in which the points range if the temperatures are by the air-thermometer. This line is inclined to the axis x of temperature, at an angle of which h is the cotangent (see 'Proceedings,' vol. vi. p. 98). At points in it corresponding to temperatures 50°, 60°, 70°, &c., straight lines are drawn parallel to the axis x; and at distances in these equal to the respective computed differences, straight lines are drawn at right angles to the axis x, and meeting the lines of constant pressure drawn through the corresponding points of the straight line which represents the gradient of density. The curved line passing through the points of intersection, is that in which the points of density range if laid off to the temperatures by the mercurial thermometer.

The author then states that the first attempt was made by obtaining the value of the constants g and h ('Proceedings,' vol. vi. p. 98) from the observation at 50° and 100°; then computing the

intermediate tensions at 60°, 70°, &c., and comparing them with observation. The result is given in the following table :—

TABLE I.

50°	60°	70°	75°	80°	90°	100°	Temp. by mercurial therm.
91·98	148·52	232·22	287·27	353·06	523·71	760	Computed tensions.
91·98	148·79	233·09	288·50	354·64	525·45	760	Observed tensions.
0	+0·27	+0·87	+1·23	+1·58	+1·74	0	Difference.

The same operations were performed with temperatures corrected. The result is given in the following table :—

TABLE II.

50°	60°	70°	75°	80°	90°	100°	Temp. by merc. therm.
50°·512	60°·481	70°·413	75°·366	80°·310	90°·171	100	Temp. by air-therm.
91·98	149·03	233·30	288·60	354·60	525·09	760	Computed tension.
91·98	148·79	233·09	288·50	354·64	525·45	760	Observed tension.
0	−0·24	−0·21	−0·10	+0·04	+0·36	0	Difference.

It is remarked that the differences in the first table show a distinct curvature with reference to the chord; while in the second table the accordance with the straight line is as perfect as could be expected.

With the view of bringing these facts out into higher relief and presenting the deflection to the eye on a scale that should at once be relatively correct and very highly magnified, the author computed six values of h from the observations at 90° and 100°, at 80° and 100°, &c., without correcting the temperatures, by the formula

$$h=\frac{t_2-t_1}{\qquad}$$

These are given in the following table; they are quantities proportional to the cotangents of the inclination of the chords to the axis of temperature.

TABLE III.

50°	60°	70°	75°	80°	90°	111°·74	Temp. by merc. ther.
158·854	159·025	159·357	159·571	159·816	160·387	165·406	Values of h.
0	+0·171	+0·503	+0·717	+0·962	+1·533	+6·552	Diff. from h at 50°.

The differences in this table are progressively increasing, and their relation to each other is very nearly that of the corresponding differences of the inclination of the chords. They are represented by these inclinations in a figure, and, in order to render the divergence from a straight line more manifest, the scale taken is 10° angular measure to the unit of difference, the length of the chords corre-

sponding with the intervals of temperature below 100°. Joining the extremities of the chords, a magnified view is obtained of the curve determined from observations with temperatures uncorrected. On this the author remarks, that if the temperatures required no correction, the points so determined would lie in a straight line, always taking for granted the integrity of the law of density and the perfect accuracy of the observations.

The next step was to perform the same computation with temperatures corrected. The resulting values of h are given in the following table:—

Table IV.

50°	60°	70°	75°	80°	90°	111°·7	Temp. by merc. ther.
56°·512	60°·481	70°·413	75°·366	80°·310	90°·171	111°·50	Temp. by air-therm.
157·121	156·983	157·006	157·053	157·155	157·428	161·738	Values of h.
0	−0·138	−0·115	−0·068	+0·024	+0·307	4·617	Diff. from h at 50°.

The difference between h at 50° in the two tables being 1·733, a straight line is drawn from the point corresponding to 100°, making an angle of 17°·33, with the chord for 50° in the uncorrected temperatures; and lines are drawn from the same point making angles 1°·38, 1°·15, 0°·68, &c. with this line, the intersections of which with the distances or chords corresponding to the temperatures, give the points which represent on the same magnified scale, the observations with the temperatures corrected. The author remarks that the line joining these points represents the *empirical* law of density, and that its relation to the standard right line for the temperature 50° is precisely what might be expected to subsist between the empirical and true curve of tension. It intersects that line—and *intersection*, not *contact*, is the character of empirical formulæ—at 50°, 75°, and 100°, and at intermediate temperatures diverges from it to the extent of about $\frac{1}{40}$th of a degree at the maximum.

Thus, he states, M. Regnault's observations between 50° and 100° afford a distinct answer to the inquiry in the affirmative, and it seems no longer possible to doubt that there is a difference between the mercurial and air-thermometers below 100°; and that its amount does not sensibly differ from the formulæ that embrace MM. Dulong and Petit's standard observations. He annexes these formulæ in a combined form adapted to the Centigrade scale.

$$t_m = \frac{Bt_a}{A - t_a} - \frac{t_a^3}{C^3} - \frac{t_a}{D} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

t_a=temperature by air-thermometer .... $\log B = 3·7145723$

t_m=temperature by mercurial thermometer $A = 4539°·617$

$\log C^3 = 6·43303$

$\log D = 0·78587$

It would be more convenient if we could express t_a in terms of t_m, but this can only be done approximatively, as in the following:

$$t_a=\frac{t_m}{\frac{B}{A-t_m}-\frac{t_m^2}{C^3}-D.} \quad \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

If greater accuracy is required, the rule is to find t_a from t_m by (2), then substitute it in (1), and compute t_m; this compared with the true value shows the alteration to be made in t_a to obtain *its* true value.

In conclusion the author observes, it might be expected, without reference to theory, that the curve deduced from the uncorrected temperatures should not show, in its continuation above 100, any abrupt divergence from its regular course; nevertheless from 100° to 111·74 the direction of the chord shows such a break in the law of continuity, and which there appears no way of accounting for, unless by a fault in the observations above 100°. Their divergence from the law of density is shown in the Chart.

The Society then adjourned over the vacation to Thursday, November 18, 1852.

November 18, 1852.

COLONEL SABINE, Treasurer, in the Chair.

In consequence of the public funeral of His Grace the late Duke of Wellington having been fixed for this day, the business of the Meeting, out of respect to the memory of the deceased, was confined to reading the Statute giving notice of the ensuing Anniversary Meeting.

November 25, 1852.

The EARL OF ROSSE, President, in the Chair.

The Rev. B. Price, and Mr. Wyndham Harding were admitted into the Society.

The following Gentlemen were elected Foreign Members of the Society:—

A. T. Brongniart.	M. V. Regnault.
Benjamin Peirce.	Dr. Lamont.

The following papers were read:—

1. "New solution of Kepler's Problem." By Professor P. A. Hansen. Communicated by G. B. Airy, Esq., Astronomer Royal, F.R.S. &c. Received November 18, 1852.

It is well known how much labour has been bestowed by geometers on the solution of Kepler's Problem, and what complicated results have been obtained for the coefficients in the expression for the Equation of the Center. I have lately found a new solution of this problem, which differs so strikingly from former solutions in this respect, that it leads to an unexpectedly simple law of coefficients. It is as follows:—

Let g be the mean anomaly;
ϕ be the angle of excentricity;
i be any positive integer number.

Put $\beta=\tan\frac{\phi}{2}$, $\mu=i\cos^2\frac{\phi}{2}$,

$$P_i=1+\mu+\frac{\mu^2}{2}+\frac{\mu^3}{2.3}+\&c.+\frac{\mu^i}{2.3\ldots i}$$

$$P_{i+1}=P_i+\frac{\mu^{i+1}}{2.3\ldots i+1}$$

&c.

$$Q_1=1-\mu,$$

$$Q_2=Q_1+\frac{\mu^2}{2},$$

$$Q_3=Q_2-\frac{\mu^3}{2.3},$$

&c.

Then the Equation of the Center

$$=(1-\beta^2)\Sigma_1^\infty\frac{2}{i}\left\{P_i.\beta_i+P_{i+1}.Q_1.\beta^{i+2}+P_{i+2}.Q_2.\beta^{i+4}+\&c.\right\}\sin ig.$$

The analysis which has led me to this, and the form of this result, are by no means peculiar to the Equation of the Center only, but apply to all functions which it is necessary to develope in series proceeding by sin ig or cos ig.

2. "An Experimental Inquiry undertaken with the view of ascertaining whether any, and what signs of Current Force are manifested during the organic process of Absorption (Lacteal) in living animals."—Part II. By H. F. Baxter, Esq. Communicated by Dr. Todd, F.R.S. &c. Received October 29, 1852.

In the experiments related in this paper, it is shown that when the electrodes of a galvanometer are brought into contact, one with the mucous membrane of the intestine, and the other with the chyle flowing from the lacteal of the same part, an effect upon the needle occurs indicating the chyle to be *positive*. The effects may be partly due to the changes which take place during *secretion*, the mesentery acting as a conducting body; this supposition however will not negative the conclusion that the effects may be attributed, in some measure at least, to the changes which occur during lacteal absorption.

3. "An Experimental Inquiry undertaken with the view of ascertaining whether any, and what signs of Current Force are manifested during the organic process of Assimilation in the Muscular and the Nervous Tissues in living animals."—Part III. By H. F. Baxter, Esq.

§ I. *On the existence of Current Force in the Muscular Tissues.*

After relating the conclusions arrived at by Matteucci in reference to the origin of the *muscular current*, the author endeavoured to obtain more direct evidence by forming a circuit between the muscular tissue and the venous blood; the effects however were but slight, they nevertheless indicated the *tissue* and the *venous* blood

to be in opposite electric states. The results of the experiments tend to confirm the inferences of Matteucci.

§ II. *On the existence of Current Force in the Nervous Tissues.*

After referring to the results obtained by Pacinotti, Puccinotti Matteucci, and Du Bois Reymond, experiments are related in which it is shown that if one electrode be inserted into the substance of the brain, and the other be brought into contact with the blood flowing from the internal jugular vein, an effect upon the needle occurs indicating the blood to be *positive*; an effect also was easily obtained if the latter electrode was placed in contact with any other part of the animal, such as the muscles. Reasoning from the results obtained, combined with physiological evidence, it was considered that they tend to establish the conclusion that the effects are due to the changes which occur during the organic process of assimilation or nutrition.

4. "On the Theory of Waves." By Andrew John Robertson, Esq. Communicated by the Rev. Henry Moseley, M.A., F.R.S. &c. Received September 6, 1852.

The author remarks that in the seventh volume of the Cambridge Philosophical Transactions, Mr. Earnshaw has a paper on the Mathematical Theory of the Waves of Translation, and that the objection to the theory therein given has been pointed out by Professor Stokes in a Report to the British Association, namely, that it requires a mathematically sudden generation and destruction of motion. Previously to his having an opportunity of reading Mr. Earnshaw's paper he had considered the subject in a manner entirely different (Phil. Mag. Dec. 1850, March 1851). The analysis he then employed was not however such as to lead to results sufficiently general, and he has in consequence now employed a higher analysis, in the application of which he acknowledges himself indebted to Mr. Earnshaw's paper for considerable assistance.

Taking the results deduced by Mr Scott Russell from his experimental inquiry, as the basis of his investigation, the author assumes, —1st, that the horizontal motion, produced by the passage of a wave, in every particle of any vertical column is the same; and 2nd, that the velocity of transmission is uniform. On these principles he deduces the value of c, the velocity of transmission,

$$c=(h\pm 2k)\sqrt{\frac{g}{h\pm k}};$$

in which h is the depth of the undisturbed water; $2k$ is the height of a positive and the depth of a negative wave; and g the accelerating force of gravity. Hence it appears that the velocity is greater for a positive than for a negative wave in the same channel.

Comparing this result with Mr. Scott Russell's experiments, it appears that on fourteen experiments of positive waves, the total error is $3\frac{1}{2}$ per cent. on the sum of all the velocities; and that on sixteen experiments of negative waves, the error is scarcely 2 per cent. on the sum of all the velocities. The author infers that it may

therefore be safely considered that the experiments bear out the theory, which shows that the positive and negative waves are phenomena of the same class, and not distinct, as maintained by Mr. Scott Russell and Mr. Earnshaw.

The next point considered is the horizontal motion of the individual particles occasioned by the passage of a wave. It is shown that the velocity of any particle is shown by the equation

$$u=\pm c\frac{2k}{h\pm 2k}\sin^2 q(ct-x),$$

in which $q=\frac{1}{c}\sqrt{\left(\frac{g}{2(h\pm k)}\cdot\frac{h\pm 2k}{h}\right)}$.

At the crest of the wave this becomes $\pm c\frac{2k}{h\pm 2k}$; and when the height of the wave is equal to the depth of the water, the velocity is $\frac{c}{2}$, or half the velocity of the wave itself. In a negative wave the motion of the particles is in the direction opposite to that in which the wave is moving.

According to Mr. Scott Russell's observations, a wave breaks when its height is equal to the depth of water, but the author considers that Mr. Russell did not succeed in producing a wave of a cusped form at once, and that it assumed that form and broke only when it travelled up a channel with a rising bottom. He further observes, that at the commencement and termination of the motion, the direction of the particle is vertical; under the crest of the wave it is horizontal; and that the path has an oval form, but evidently not an ellipse. He next deduces λ, the length of a wave, in terms of T, the period of the wave, or the time which elapses from the commencement to the termination of the motion of a particle, viz.

$$\lambda=c\mathrm{T}\sqrt{\frac{h}{h+2k}};$$

on which equation he remarks, that setting aside the variation caused by a change in the value of k, both in that of c and in the radical, the length of the wave varies directly as T; and that we have thus an explanation of the observed fact, that a wave becomes gradually diminished in height and increased in length.

The author then enters on the subject of *Oscillating Waves*. He remarks that, in the wave of translation, it appears from the preceding investigation, that the horizontal and vertical motions of the particles commence and terminate together; and that consequently the particles left at rest when the wave has passed must continue in the same state, unless some fresh disturbance again set them in motion. But if the vertical motion were at its greatest when the horizontal motion was destroyed, the particles would oscillate in both directions, and a disturbance once given would continue until friction or external force destroyed it. He further observes, that it has been shown by Mr. Airy (Encyc. Metrop.) that the motion of the particles

in oscillating waves diminishes in a geometrical ratio as their distance below the surface increases, which result agrees with Mr. Scott Russell's observations; but that it is interesting to examine whether a series of oscillatory waves can exist in a channel of uniform breadth and depth, upon the supposition that the horizontal excursion of the particles is the same from the top to the bottom. From the analytical investigation of the author, it appears that there can exist in a channel of uniform breadth and depth a series of waves of the same magnitude and moving with the same velocity. In the case of rivers, the tide-wave appears to be of this description. If the effect of friction on the bottom be neglected, the horizontal velocity of the particles may be considered to be the same throughout the whole depth—and the motion is oscillatory, if that due to the stream be separated. But the length of the wave is very great in comparison with the depth, and the vertical motion is so slight, that it may be neglected altogether. On this supposition, which simplifies the investigation, the velocity for a wave of translation is

$$(h+2k)\sqrt{\frac{g}{h}};$$

and for an oscillating wave is $(h+k)\sqrt{\frac{g}{h}}$;

which differ but little from the results previously obtained, and are rather in excess, as was to be expected, when no part of the force has been expended in producing vertical motion.

Referring to the solution of this problem, given by Mr. Airy in the Encyclopædia Metropolitana, and the equation there arrived at, the author states that it is with great diffidence, and not until after the most careful examination, that he ventures to question the accuracy of this result. In a further investigation, he points out the source of this inaccuracy; and in conclusion observes, that it must be confessed to be unsatisfactory to point out an error without supplying the deficiency caused by it, but that the analytical difficulties are such, that he cannot, consistently with the attention required by other avocations, attempt, for the present at least, to continue an investigation which is interesting as a matter of science and useful in its application to hydraulic engineering.

November 30, 1852.

At the Anniversary Meeting,

The EARL OF ROSSE, President, in the Chair.

Dr. Booth, on the part of the Auditors of the Treasurer's Accounts, announced that the total receipts, during the past year, including a balance of £147 8*s*. 6*d*. carried from the account of the preceding year, amounted to £3528 5*s*. 1*d*.; and that the total expenditure, including an investment of £115 0*s*. 0*d*. in the Funds, was £3346 5*s*. 0*d*., leaving a balance in the hands of the Treasurer of £182 0*s*. 1*d*.

The thanks of the Society were voted to the Treasurer and Auditors.

List of Fellows deceased since the last Anniversary.

On the Home List.

John George Children, Esq.
William Tierney Clark, Esq.
Major-Gen. Thomas Colby, R.E.
Augustin F. B. Creuze, Esq.
John Dalrymple, Esq.
George Dollond, Esq.
Capt. C. M. Elliot, M.E.
Sir Josiah John Guest, Bart.
The Duke of Hamilton.
Lieut.-Col. George Hutchinson.
William Jacob, Esq.
Capt. Thos. Locke Lewis, R.E.
Rev. William Forster Lloyd.
Gideon Algernon Mantell, LL.D.
Sir John Henry Pelly, Bart.
George Richardson Porter, Esq.
John Corse Scott, Esq.
William Ford Stevenson, Esq.
Sir Edward Stracey, Bart.
Dr. Thomas Thomson.
Charles Augustus Tulk, Esq.
Peter Evan Turnbull, Esq.
Rev. John Warren.
Sir Frederick Beilby Watson.
The Duke of Wellington.
George Wilbraham, Esq.
Glocester Wilson, Esq.

On the Foreign List.

Paul Erman.

Withdrawn.

The Very Rev. the Dean of Chichester.

List of Fellows elected since the last Anniversary.

On the Home List.

Arthur Kett Barclay, Esq.
Rev. Jonathan Cape.
Arthur Cayley, Esq.
Henry Gray, Esq.
Wyndham Harding, Esq.
Arthur Henfrey, Esq.
John Higginbottom, Esq.
John Mercer, Esq.
Hugh Lee Pattinson, Esq.
Rev. B. Price.
William Simms, Esq.
Hugh E. Strickland, Esq.
John Tyndall, Esq.
Nathaniel Bagshaw Ward, Esq.
Captain Younghusband, R.A.

On the Foreign List.

Adolphe Théodore Brongniart.
Dr. J. Lamont.
Benjamin Peirce.
Victor Regnault.

The President then addressed the Society as follows:

Gentlemen,

For more than two years the experiment has been tried by your Council, and on a considerable scale, of promoting original research by direct encouragement. The time has now arrived, when, although in most cases the researches are unfinished, still considerable progress having been made, it seems fitting that some account should be

rendered to the Society of the measures taken, and the results obtained.

At an earlier period little could have been said except as to our hopes and expectations: we can now point to results; and each succeeding year, as researches draw to a close, new matters of interest will arise requiring your especial notice.

You have all no doubt observed that as science advances, truth has to be sought out under every variety of circumstances. Sometimes the investigation is easy and inviting, at other times laborious and perhaps repulsive. When there is an immediate prospect of striking discoveries, the interest of the subject brings many into the field, and there is often even a vigorous contest for priority; where however the only prize to be obtained is a few dry facts, important in themselves, as opening the way to further progress, but otherwise perhaps of little interest, direct encouragement is necessary. We cannot overrate the importance of collecting facts: the whole history of the inductive sciences shows that without facts discovery cannot progress; that we must, in fact, work the rock if we wish to extract the ore. Where there is much labour and little fame in collecting facts, an impulse is given by associations; the labour is undertaken, and the work accomplished; but often more than labour is necessary: the facts are not to be obtained without cost, and the cost may be much too great to be conveniently borne; it is then that the Council assists; the object is accomplished; and an opening made for the farther advance of scientific discovery. In disposing of the Government Grant, your Council have endeavoured to take a wide view of the interests of science; and therefore in constituting the Committee to decide upon the applications for assistance have passed their own limits, and have associated with themselves an equal number of fellows, selected so as to ensure to each department a fair representation. They were thus made aware of the views and wishes of other Societies, and in more than one instance they have been requested by the British Association to assist that body in researches which seem entitled to national aid. By thus administering their trust, more is effected than the mere results that are obtained by its assistance, for it leads to a more perfect union, a more effective co-operation among men of science, and it tends to place the Royal Society in the position which it was originally designed to hold,—the centre of the system of our intellectual universe, round which the other luminaries of this land pursue each its own orbit.

In applying this grant for the present year, the same principles as in previous years have been taken as a guide, viz. the value of the results expected, and the improbability of their being obtained without pecuniary assistance. Two grants have reference to a branch of Physics still very obscure, but every day increasing in importance; the relation of the molecular actions which formerly were attributed to imponderable fluids, but now are generally considered modes of motion or power. Grove's views as to their correlation are familiar to you, and are now in fact receiving a rigorous verification from the researches of Joule and Thomson. The first has shown by elaborate

experiments that heat has a definite equivalent of mechanical power, and also of electrical current; and the other, applying to these facts the resources of theory, has extended this principle to chemical forces with fair promise of success. The experimental verification of his reasoning, and the collection of numerical data, promising as they seem to do, no less than the power of including the whole doctrine of affinity within the range of calculation, appear of such importance, that the Committee have considered the subject worthy of a grant, and have devoted to it a sum of fifty pounds, the amount required.

They have also placed thirty pounds at the disposal of Mr. Joule for an analogous purpose, the investigation of the change of volume which takes place in iron on which magnetism is induced, indicating, as it seems to do, a close connection between that energy, and the tensile and compressive forces of the metal.

A grant of fifty pounds has also been made to Dr. Tyndall, to examine the conducting power of crystals for heat, as compared with their transmission of it. In this last respect, as was long since shown by Melloni, they differ widely; thus rock salt is almost perfectly diathermic or transparent for heat; alum as decidedly the reverse; but the propagation of heat by radiation differs so widely from that by conduction, that it is important to inquire, what differences exist as to the latter; and the more so, because it has been shown by de Senarmont, that in most crystals the conducting power varies in different directions.

An equal sum has been granted to Mr. Dale for researches in the same direction as those of Dr. Kohlrausch (lately made known to the English reader by Dr. Tyndall's translation), respecting the electric tension of the various parts of the voltaic circuit. They belong to the most refined and delicate class of observations, and are peculiarly open to various causes of deception; while the information to be derived from them is of an order, whose value is only now beginning to be fully appreciated. It is a very narrow view of their value to regard them merely as criteria between the contact or chemical theory of the voltaic battery; that question will apparently be soon forgotten in the wider system, to which I have already alluded as indicating the correlation of all these corpuscular forces. Every new fact, every wider glance which we obtain over the world of physics, shows that man can only transform, or modify force, but not create it: even when his own will generates motion in his own body, it does so only by the subversion of chemical affinity, in the components of that body, and so in every other instance. The voltaic current, whatever it may be, can do a continuous and definite amount of chemical or calorific work; its power therefore must be derived from an equivalent and continuous expenditure of some other force. Such an expenditure, as Faraday has well remarked, cannot be afforded by mere contact, but is unequivocally presented by the chemical action of the battery. In the common electrical machine the friction seems to act in producing a succession of changes in the elastic forces of the rubbing surfaces, and the intense electricity thus evolved, shows by its difference of

character from the voltaic, the difference of the force from which it proceeds; mechanical power acting by intermitting pressure. This force also, through the medium of the magnet, produces a similar effect; and heat when applied to crystalline or metallic bodies, originates peculiar electric phenomena. Doubtless similar results will also be discovered to proceed from the action of capillary attraction, the disintegration of solids, or the change of density in fluids. Many of these it is probable are concerned in producing the electroscopic phenomena observed in the voltaic circuit, which are by no means necessarily connected with the true current; and therefore independent of mere controversy, it is important that they should be carefully observed and measured. In Mr. Dale's hands we have not only every reason to believe that this will be effectually performed, but that he will also advance and improve the means of making this class of observations which at no distant date must play an important part in molecular physics.

As to Prof. Williamson's grant for investigating the law of the chemical action of masses, Berthollet carried this to an extreme, believing that in every case mass was as important as affinity, so that the strongest force of the latter would be compensated by a larger amount of the former acting by a very weak chemical attraction. This was quite at variance with any theory of definite proportions, and he explained the multitude of instances which seemed to enforce that theory by the influence of cohesion, crystallization, and other molecular forces. This view, though ingenious and containing some truth, has been thrown into the shade by the triumph of the other; but Mr. Williamson believes that he has found in the class of compounds represented by sulphovinic acid, a means of measuring the influence of quantity,

This year a grant has been made, to the same amount as last year, to defray the expense of drawings of interest, to be executed under the direction of Professor Owen: the valuable result of the previous grant, by which a series of beautiful drawings of the skeleton of the Megatherium in the British Museum were obtained, make it desirable that you should continue to profit, in the same way, by his powers and genius.

As to the grants of former years, where the investigations are incomplete, satisfactory reports have been received as to the progress made; where complete there is every reason to be satisfied with the results. Among the latter are Mr. Cooper's Catalogue of 14,888 ecliptic stars. It is a most valuable addition to Astronomy, going down to stars of a magnitude far below that which were previously observed; the limit being those visible in the unilluminated field of an achromatic of $13\frac{7}{10}$ inches aperture. In the Introduction to this work the mode of observation is detailed, and the limits of error within which the places may be affected for a single observation is $1''\cdot3$ or about $6''$ in space; but as many of them have been thirty times observed, and most of them from fifteen to twenty times, the probable error of the places given in the Catalogue may be taken at $2''$. When the maps he is constructing from this Catalogue are

completed, we shall possess a knowledge of the zone comprised in six degrees on each side of the Ecliptic, which will ensure the detection of even the minutest asteroid. The scale is 2 inches to the degree, and in some squares there are 150 stars.

I regret to say that the ill health of Dr. Robinson, and the long illness of his principal assistant, have delayed the publication of the Armagh observations, for which a grant was made. Eight hours of right ascension are now ready for the press, but a much more than proportional part of the remainder is completed. The Astronomer Royal has prepared the tabular portion of Mr. Catton's observations, and the introduction is nearly ready, so that the work will be soon in the printer's hands.

The two grants for the examination, the preparation, and the correction of meteorological instruments at the Kew Observatory, appear to have been very successful. To provide the means of testing the many new instruments which are offered to scientific men, so that their real merit may be ascertained, and waste of time and disappointment be prevented, is to render a very important service to science. Regnault's apparatus, recently purchased, has already supplied Standard Thermometers for Government expeditions, and the Committee have received numerous applications for Standard Thermometers constructed with it, from distinguished physicists, and the principal opticians. Dr. Carpenter has completed his drawings of Foraminifera, ninety in number, and Professor E. Forbes has reported so favourably of them that I trust they will soon be published.

A subject of considerable interest has been taken up by Mr. Horner, geological researches in Egypt; the question at issue being the rate at which the alluvial land of the Nile valley was formed, from the first cataract to the sea. The French at the end of the last century made out what they considered to have been the rate of secular increase, about six inches in a century; and the object of the present researches is to ascertain whether that average rate may be assumed to be correct, and if so to apply the scale to vertical sections of the Nile deposit, obtained by sinking pits in the immediate vicinity of monuments, the age of which is known. Extensive researches were made in the summer of 1851, liberally aided by Abbas Pacha, on the site of the ancient city of Heliopolis, and during the last summer on the site of the ancient city of Memphis. The operations have been conducted by an engineer in the Pacha's service. Mr. Horner has had several letters from him describing the operations at Heliopolis: he has also received specimens of the layers of soil passed through in the nine sinkings at Heliopolis, a specimen from the deposit of the Nile water at the time of inundation. Some of the soil from the Barage, and some specimens of Nile water have been analysed under the direction of Dr. Hofmann, at the Royal College of Chemistry. Detailed reports have not yet been received from the engineer, either as to the operations at Heliopolis, or at Memphis; until then it would be premature to form any conclusions.

The success of the system of self-registration as applied to mag-

netism, and meteorological instruments, and to which Mr. Brooke had contributed not a little, induced him to consider the practicability of contriving apparatus to render the force-magnetometers self-correcting. At present, as the temperature of the magnet varies, a correction is required which is sometimes considerable, and therefore the records of magnetic indication, and of the temperature of the magnet, should be simultaneous to afford proper data for accurate reductions. By this method the labour of deducing results from the photographs is very considerable, and it occurred to Mr. Brooke that an efficient automatic compensation might be applied to the magnet, in the same way as the compensation of the balance of a chronometer. To carry out the necessary experiments a grant was made, and a paper with drawings and descriptions of the apparatus constructed was presented to the Society. The apparatus itself was deposited in the Great Exhibition, as offering satisfactory evidence of the advanced state of magnetic science in this country. Recently three sets of instruments have been made; one set has been sent to Washington, another set is in preparation for the Ecole des Arts et des Métiers at Paris, and a third set for the Cabinet de Physique at Florence.

Mr. Miller's interesting researches on the rain-fall in the lake districts have been completed, and the results communicated to the Society.

A small grant was made to Dr. Thurnam for the drawings of crania exhumed from tumuli, as a basis for certain ethnological inquiries. The drawings have been nearly completed, and Dr. Thurnam hopes in the course of a few months to give so much time to the subject as will be sufficient for the proper arrangement of the drawings, and for describing the evidence on which their appropriation depends.

In the disposal of the Government Grant chemistry has not been forgotten; grants have been made to the two distinguished chemical philosophers, Dr. Stenhouse and Dr. Hofmann. You are no doubt aware that Dr. Stenhouse has been engaged for a number of years in an extensive series of researches into the chemical relations subsisting among the various genera of plants; the chief aim of these inquiries has been the illustration of Botany by means of chemical science. Some of these inquiries have already appeared in your Transactions. The grant has been made to assist in defraying the very heavy expenses which are unavoidable, consisting of the cost of materials, and the salaries paid to qualified assistants.

Dr. Hofmann, in two papers published in your Transactions, has endeavoured to establish a general theory of the constitution of the organic bases, by developing the nature of the relation in which these substances stand to ammonia, and the hypothetical compound ammonium. He has pursued these inquiries very much farther; and recent experiments have shown that his former views were correct, the facts discovered perfectly harmonizing with theory. There remain still two lines of inquiry, the constitution of some of the fixed crystalline bases, and that of the alkaloids. The alka-

loids are very expensive materials, and the principal object of the grant was to meet this expense. The last grants I have to notice are to Professor Stokes, Lucasian Professor of Mathematics at Cambridge, for experiments to determine the index of friction in different gases; and to Professor Hopkins, of Cambridge, for investigations on the effect of pressure on the temperature of fusion of certain substances; both inquiries are attended with great difficulties, but the results cannot fail to be of the highest interest. In each case apparatus of an expensive character was required, which has recently been completed; and some progress has been made in the preliminary experiments, but a short time will necessarily elapse before the results shall have been ascertained with sufficient certainty to fit them for communication to this Society.

I have thus given a very slight sketch of the various inquiries which have been completed, or are in progress, aided by the Government Grant. In all cases the notice has been as brief as possible; still I trust enough has been said to show that the efforts made by your Council to employ to the best advantage the means at their disposal, have not been unsuccessful.

The prospects of science are brightening in all directions: the many recent applications of science to utilitarian purposes have satisfied the masses, that science is not a mere unprofitable abstraction: the progress of knowledge dispelling error, seems to have dissipated the delusion that in science there might possibly be something ungenial to our institutions and to stable government. The feeling seems rapidly to be gaining ground, that our place, as a nation, will depend in no small degree upon our success in seizing upon the truths of science, and applying them; that in fact it is true that knowledge is power. It is therefore not improbable that unusual efforts will be made to give an impulse to scientific research of every kind, and that this Society, taking the place it has ever held, at the head of English science, will be called upon for renewed efforts, For that place your Society is more than ever fitted, as the fellowship stands much higher than it ever did before. The merits of the candidates for fellowship are tested with severity, but with strict justice, and consequently the fellowship is a real warrant of merit. I need perhaps hardly add, that at every election we see our ranks recruited by men who hold the first place of eminence in theoretical and practical science.

Chevalier Bunsen,

I am most happy to have the honour of committing to your care the Copley Medal for Baron Humboldt.

The Royal Society have awarded him the highest honour which it was in their power to confer, to mark their sense of the great value of his contributions to Terrestrial Physics during a long series of years. Your Foreign Secretary has very recently drawn up for the Council an account of Baron Humboldt's researches: that has been printed, and therefore it will be unnecessary for me to go over the same ground again. It is enough to say that there is no one ac-

quainted with the present state of magnetism, of zoology, of botany, of geology, or of physical geography, who is not aware of the extent and value of Baron Humboldt's researches. A scientific traveller of the highest order, he zealously endeavoured to advance the science of physical geography in its widest sense, regardless of toil and expense, and at great personal risk. Distant regions of the globe were in turn his habitation, and with remarkable patience, and a sagacity peculiarly his own, he sought out Nature's laws under every modification of climate. The mass of facts which he has given to the world, carefully arranged and discussed, constitute a mine of information from which Cosmogonists will long continue to draw with profit; while in its vastness it will be regarded with astonishment as the work of one man.

The Chevalier Bunsen then replied as follows:—

My Lord President,

This occasion is one of great solemnity, and to me one of almost overwhelming emotion. The most ancient and illustrious scientific institution of Europe has awarded its highest honour to the Nestor and Prince of the men of science of my country. The Council of this Society have done me the honour to propose to me to receive that Medal in the name of my illustrious friend, and he has been pleased to signify that this arrangement is gratifying to him. I may add, that I know that the King whom I have the honour to represent in this country, takes a deep interest in this occasion.

Nobody, I am sure, appreciates more fully than that great man himself, the value of the approbation of England and English science expressed in your award. It so happened that Humboldt's first immortal efforts were made in the time when England was almost entirely separated from the Continent; insomuch that he came hither not earlier than in 1818. The interest which England took in him and his works has since that time been sincere and constantly increasing. I only repeat what he has often expressed to me in his letters and by word of mouth, if I say, that he feels this increasing general interest as one of the blessings of his old age. Your scientific men knew him for half a century as one of those heroes to whom Science owes gratitude for eminent services—the geographer and the botanist, the physical and the historical philosopher, the geologist, the astronomer and the zoologist.

But the great national interest dates from a later time; and what could be more satisfactory to him, than that its organ should be, on so solemn an occasion, that Society which for more than 200 years has taken the lead in so many branches of science, a Society which counts Newton among its most active members; which has first applied Science to History and to Antiquities; which has carried the torch of scientific inquiry into the recesses of the Pyramids and the night of ancient chronology; which has made the first successful efforts in the unrolling of the Greek Papyrus rolls of Herculaneum, and which has elucidated so many important points of Roman topo-

graphy connected with scientific measurements and observations: a Society which now counts amongst its members so many luminaries of science, and which possesses a President who is not only a Peer of the realm, but also a Peer in the realms of the republic of intellect and science?

I now leave my venerable friend, in order to express to you my thanks for the honour done to him, in the name of my country and of German science. For in honouring him you have honoured both: you have honoured a man to whom Germany looks up with undivided respect and with just pride.

The admirable sketch given of his literary efforts and successes by the Council of this Society, shows good cause for this respect and this pride. We admire in him, above all, the unity of thought and of purpose which pervades this long and illustrious life, and the perseverance with which he has, in his maturer age, concentrated the pursuits of his youth, on the basis of his own unrelenting studies and uninterrupted observations. As Herodotus, after he had travelled over the greatest part of the civilized portion of the ancient world, comprehended the results in his immortal History, thus Humboldt, after having observed the phænomena of nature from the Chimborazo to the frontiers of China, concentrated his thoughts and researches in his immortal *Kosmos*.

In ordinary cases, we might have been led to expect of such an all-comprehensive work, a spirited aggregate of his own thoughts. and those of others; yea, some people might have regretted that the author had not confined himself to a smaller field, to be more original. But Humboldt attempted something higher, and by the consent of mankind he has accomplished it in a very eminent degree. He thought that he could show why and how this World and the Universe itself is a *Kosmos*, a divine whole of Life and intellect, namely, by its all-pervading *eternal laws*. Law is the supreme rule of the Universe: and that Law is Wisdom, is Intellect, is Reason, whether viewed in the formation of planetary systems or in the organization of the worm; and Man is the Microcosmus and centre of this creation, contemplating, and more or less perceiving, this universal order; and science is called upon to investigate and to interpret them as far as she is able. This work, in short, is not a *farrago*, it is an original composition, part of which is illustrated by the rest, and the whole of which is greater than all its component parts together. Humboldt began the fourth and last volume when he entered his 82nd year, and his last letter, only a few days old, informs me of the progress of the printing of that volume.

My Lord, it is for these reasons that I beg to express to you the thanks of my country and of German science which you have honoured in Humboldt.

But you have honoured more in Humboldt—you have honoured *Humanity*; for Humboldt ever has been a true cosmopolite as well as a good patriot; he has ever been a friend of mankind. Every pilgrim of Science has always been welcome in his house and his mind. Almost 40 years ago he took me kindly by the hand, when

I came to Paris for my literary studies ; he has done so by hundreds. How many youug men in science and art has he not been the first to encourage! How many great institutions and researches has he not first conceived and helped to organize without any reference to his personal comfort or honour! And there is to me, above all, that one most striking fact for the honour of our race; it is this, that his heart as well as his mind has never ceased to become larger. It is a general observation, that arrived at a certain age, men shut themselves up against the outer world, their interest decreases, their sympathies grow fainter. Not so with Humboldt: his soul has always expanded, his interests and sympathies for every great national and human interest have always been warmer, his care for the welfare of the rising generation always more affectionate, his hopes for the future of the world always brighter. I know no one more youthful and hopeful mind than Humboldt's, and I therefore may well say in every respect, that in honouring him you have not only honoured my country and science in general, but *Humanity*.

It is for these reasons, my Lord, that I beg earnestly to thank you.

The President then proceeded as follows:—

Mr. Stokes,

It is with sincere pleasure I discharge the duty which has devolved upon me of placing in your hands the Rumford Medal. Your discoveries in Physical Optics during the last few years, which have shown in so striking a manner the powers of analysis in bringing the abstruse phenomena of light within the domain of theory, have been crowned by a discovery even more important. That the refrangibility of light should be actually changed by dispersion within certain media, and that the invisible rays of the spectrum should thus be rendered visible, is a discovery as curious, perhaps as important, as any to be found in the recent history of optical science.

I am sure I but express the feeling of this meeting, when I say I confidently hope that a career commenced so brilliantly, may in its course be distinguished by other discoveries of equal value, and that you may contribute still further to extend the fame of that celebrated university where you received your education, and for which you are now making so signal a return.

Mr. Joule,

In the slight sketch I have already given of the progress of different departments of science under the aid of the Government Grant, I have noticed your labours in conjunction with the other eminent men engaged in the wide field of Molecular Physics.

The subject to which you have especially devoted your energies, the discovery of the mechanical equivalent of heat, is one which unites in itself a practical, as well as a deep scientific interest. To have carried out with great ingenuity and perseverance a series of beautiful and conclusive experiments, is an important achievement, and I am most happy it has found its reward in the Medal which I have the pleasure of presenting to you.

Mr. Huxley,

The Royal Medal for Physiology has been awarded to you for your papers 'On the Anatomy and Affinities of the Medusæ.'

In those papers you have for the first time fully developed their structure, and laid the foundation of a rational theory for their classification, demonstrating with the greatest success the mutual relations of their different groups, and their affinities to other animals.

In following out these investigations, you have availed yourself with extraordinary perseverance and intelligence, and with corresponding success, of the opportunities afforded you for the examination of these animals whilst living, by your position as surgeon to H.M.S. Rattlesnake during her surveying voyage, conducted by the late Capt. Stanley on the Coasts of Australia and New Guinea.

The results of these researches have been in part made known in the papers for which the present Medal has been awarded, and in others communicated to the Royal and other Scientific Societies. It would be difficult to give even an outline of the discoveries there made without entering into unnecessary detail, but it may be well to observe that in your second paper in the Philosophical Transactions 'On the Anatomy of Salpa and Pyrosoma,' the phenomena commonly embodied in the term "alternation of generations,' as referred to the former genus, which, from the first suggestion of Chamisso have excited so much attention, and produced so much unsatisfactory discussion, have received the most ingenious and elaborate elucidation, and have given rise to a process of reasoning, the results of which can scarcely yet be anticipated, but must bear in a very important degree upon some of the most abstruse points of what may be called transcendental physiology.

Among the list of Fellows whose death we have to deplore during the present year is that of Mr. William Tierney Clark, also a Fellow of the Society of Civil Engineers. He was the constructor of many important works in this country, such as—

1. The Thames and Medway Canal.
2. The Cast-iron Town Pier at Gravesend.
3. The Suspension Bridge at Hammersmith.
4. The Suspension Bridge at Marlow.
5. The Cast-iron Bridge over the Avon at Bath. Besides many useful works of minor importance.

He made a magnificent design for a Suspension Bridge of the River Neva at St. Petersburgh, for which he received a large Golden Medal from the late Emperor Alexander of Russia. But his last and most splendid work was the great Suspension Bridge over the Danube at Pesth, in Hungary. The many political circumstances attached to the origin of that bridge through the Count Széchenyi, the great reformer of Hungary, the subsequent dangers of destruction which the bridge escaped during the wars which desolated Hungary, and the great difficulties which attended the execution of the undertaking, independently of its importance as a work of art, have given a European celebrity to that bridge above all others of its kind.

Mr. Clark commenced his career first in Bristol and afterwards with Messrs. Darby and Co. at their celebrated iron-works at Colebrook Dale in Shropshire, where he doubtless acquired the first principles of Cast-iron Engineering, and which were afterwards more matured when he entered the service of the late Mr. Rennie in 1808, where he remained until 1811, when he was appointed Engineer to the Water-works Company at Hammersmith, in whose service he remained until the day of his death on September 22, after a long and painful illness. Of his works it may be said that they combined great elegance with good sense in the arrangement of the details.

John George Children, Esq. was born on the 18th of May, 1777, at Ferox Hall, Tunbridge. His father was the possessor of large landed property near Tunbridge, and was a Bencher of the Middle Temple, but never practised at the bar. Mr. Children's mother died a few days after his birth. His father did not marry again, but devoted himself to the care of his son, who received the rudiments of his education at the Grammar School at Tunbridge, and subsequently at Eton, on quitting which, he was entered a fellow-commoner of Queen's College, Cambridge, in 1794. His views were at this time directed to the church as his profession, but having had the misfortune to lose his wife, a granddaughter of Governor Holwell whom he had married as soon as he was of age, he accompanied some intimate friends to Lisbon, and after a residence there of some months, he returned to England, and in March 1802 sailed for North America, where a cousin to whom he was much attached had established himself. They travelled together through not only the more settled towns, but among large tracts of the then uncleared backwoods, both of the States and Canada. The change of scene had a beneficial effect on Mr. Children's spirits, but had nearly cost him his life. He was attacked by a terrible fever, and it required the most judicious treatment of his medical friends to save him. As soon as he was sufficiently recovered he returned to England, entirely restored to health by the voyage. He found his native county, Kent, busy in organising national defences, at that time the great object of attention, and he entered the West Kent Militia as one of its captains; which post he retained until 1805, when severe illness obliged him to resign.

From this period his time was principally devoted to science, to which he had been from his early youth greatly attached. Mineralogy, chemistry, and galvanism, became his favourite studies, and he soon made the acquaintance of the leading men of science. From their society he derived the highest gratification, and he lived much among them. Sir Humphry, then Mr. Davy, Mr. Hatchett, Dr. Wollaston, and many more great names of that day were among his intimate friends, and his election as a Fellow of the Royal Society in 1807, was at once the result and the cause of increasing attachment to his scientific pursuits. He had an excellent laboratory at Tunbridge, where he constructed a galvanic battery, with a small series of very large plates, of which he gave an account to the Royal

Society in November 1808; and subsequently he constructed another with much larger plates, the performance of which is fully detailed in a second paper read to the Society in June 1815. Both these papers are printed in the Philosophical Transactions.

Between the date of these papers he made a long journey in Spain, and visited the quicksilver mines of Almaden, with which Englishmen were at that time very imperfectly acquainted. On his return to England in 1809 he married the eldest daughter of George Furlong Wise, Esq., of Woolston in Devonshire, but he again experienced the heaviest of all domestic calamities by losing her within eight months of their marriage. After her death in 1810, he continued to reside chiefly with his father at Tunbridge until the year 1816, when, in consequence of the failure of the Tunbridge Bank, in which his father was a partner, his prospects in life were wholly altered, and he found it necessary to seek some remunerative employment that might enable him to contribute to the comfort of his revered and now aged parent. He succeeded, through the kindness of the late Marquis of Camden, in obtaining the situation of one of the librarians of the British Museum, in the department of Antiquities. He still retained his love for chemistry, and a little before his appointment to the Museum, he had warmly espoused the cause of his friend Sir H. Davy, in a controversy respecting the safety lamp; a paper relating to which will be found in the Philosophical Magazine for 1816.

After his father's death Mr. Children married the widow of the Rev. Johnson Towers, and removed from Chelsea to the British Museum, in which establishment he had an official residence. It is worthy of mention, that after he had been for some years an officer of the Museum, his post was changed without his own solicitation from the Department of Antiquities to that of Natural History.

In 1826 Mr. Children was elected Secretary of the Royal Society in the place of Mr. Brande. He resigned in 1827, but was re-elected in 1830, and remained in office until 1837, when his delicate health obliged him to relinquish it. This honourable position was rendered particularly agreeable to him by the regard and kindness of his colleagues, and of the Presidents under whom he acted; and his zeal for the interests of the Society is commemorated by the unanimous thanks of the Society having been given to him in 1835, for "the zeal and ability which he uniformly displayed, and the many valuable services he rendered in promoting the objects of the Society."

On his retirement from office in Nov. 1837, the President, then the Duke of Sussex, in his Anniversary Address, alluded in a very marked and complimentary manner to Mr. Children's services as Secretary, and lamented that the Society would no longer have the benefit of those services.

At this period of his life Mr. Children was a member of most of the scientific bodies of Great Britain, and of some foreign societies, and he was very instrumental in the formation of the present Entomological Society, and became its first president.

He published two chemical works, one a translation of 'Thenard's

Essay on Chemical Analysis,' 8vo, 1819; the other of 'Berzelius' Treatise on the Use of the Blow-pipe,' with additional experiments and notes of his own, 8vo, 1822. He was one of the early editors of the Zoological Journal, and a contributor to other learned works. In short, his occupations were many and varied, but they were congenial to his active mind. His knowledge of Chemistry became a source of profit to him in the year 1824, when the mining companies of South America were desirous of finding some means by which silver might be extracted from its ores without amalgamation. Mr. Children having directed his attention to the subject, succeeded in discovering and perfecting a process by which the silver might be obtained without the use of mercury, and at a less cost. The right of using this process was purchased by several mining companies, and a considerable sum was the fruit of it.

Mr. Children remained at the British Museum until the death of his wife in 1839, when he sent his resignation to the trustees. He then went to reside with his daughter at Halstead Place, Kent, who had married the only surviving son of the late John Atkins, Esq.

Although retiring from active life, he was nevertheless constantly employed in scientific researches, and he took up the science of Astronomy with the energy and zeal of a young man. Thus passed the latter years of his life, until with unimpaired faculties the powers of nature gently gave way after the brief illness of a week, and he died on the 1st of January 1852, without the slightest apparent suffering.

Thomas F. Colby, LL.D., Major-General in the Army, and one of the most distinguished scientific officers of the corps of Royal Engineers, was born at Rochester on the 29th September, 1784, and was the eldest son of Major Thomas Colby of the Royal Marines, an officer who was severely wounded at Lord Howe's battle of the first of June. His grandfather was Mr. Colby of Rhoseygilwin in South Wales, a gentleman of considerable landed property. His maternal uncle was General Hadden, Surveyor-General of the Ordnance.

The life of General Colby was eminently scientific, and its history will be hereafter embodied in that of the Ordnance Survey. He was however not less distinguished for the genuine simplicity of his character, his urbanity, his frank but unostentatious hospitality, and his private and domestic virtues, than for his scientific attainments, and the ability and energy he displayed in the performance of his public duties.

He was educated at Northfleet School under Dr. Crackell; thence admitted a cadet in the Royal Military Academy; and in December 1801, at the age of little more than seventeen, was promoted to a second lieutenancy in the Royal Engineers. He was early associated with Lieut.-Colonel Mudge (afterwards General Mudge) in the Trigonometrical Survey of Great Britain, that officer having become acquainted with his mathematical acquirements and tastes. In 1811, was published the third volume of the Survey, which contains

"an account of the Trigonometrical Survey, extending over the years 1800 to 1809, by Lieutenant-Colonel William Mudge of the Royal Artillery, F.R.S., and Captain Thomas Colby of the Royal Engineers." This association of his name with that of the Director of the Survey, of itself shows the active part he thus early took in the operations of that important undertaking, and the estimation in which his services were then held. He was elected a Fellow of the Royal Society in 1820, and in 1821 obtained the brevet rank of Major.

In 1824 Major Colby commenced the great work of his life, the Ordnance Survey of Ireland. In that year a Committee of the House of Commons reported on the necessity of a General Survey of Ireland, and recommended that it should be undertaken by the Ordnance. The Duke of Wellington was Master-General, and having assumed the responsibility of such a task, he confided its execution to Major Colby, who had then for some years conducted the Survey of Great Britain. The survey required for Ireland was very different in its nature and objects from that of Great Britain: it was expected to be laid down and published on a scale of 6 inches to a mile, and was designed to form the basis of a land valuation, and of a revised system of local taxation. For a work of such minute detail and such close precision, as these objects rendered necessary, Major Colby was obliged to create the means of execution and to devise a plan of operations which should enable him to employ numbers as well as skill. Taking for his model the celebrated 'Down' Survey of Sir William Petty (subsequently so well described by Major Larcom, R.E), and applying the whole energies of his mind to the subject, he devised that beautiful system of disciplined and co-operative labour which enabled him to apply to the work all the resources of science, and yet to employ upon it both private soldiers and peasants. The Royal Sappers and Miners supplied the highly-trained soldiers who formed a nucleus for the work, and the quick and intelligent peasantry of Ireland produced numbers of candidates sufficiently instructed to serve as materials for its perfect construction. To those who saw the work in its infancy, when everything had to be created, and remained to witness it as a vast and beautiful machine, combining into harmonious action the labours of about forty observers and many hundreds of surveyors and draughtsmen, and producing annually a perfected survey of several millions of acres, the success of General Colby must appear most complete and most wonderful.

To secure the undisturbed and uniform movement of so complicated a machine, it was necessary to form an equally perfect office establishment, and this was done in the Survey Office at Mountjoy, in the Phœnix Park, Dublin, the arrangements of which (including those of the Engraving Establishment) were carried by Colonel Colby to the utmost perfection, under the personal superintendence of Captain (now Major) Larcom; and the final excellence of that establishment may be now studied in that of the Map Office at Southampton, which is, in fact, no more than its reflected image.

Having thus grasped in his mind the requirements of the survey in its mere practical character, Major Colby felt, as a man of science, that so great a national work ought not to fall short of the excellence of Continental works in any of its operations, and that some scientific advance should be made in the mode of measuring its first base line. With the beautiful differential rods of the French philosophers he was not perfectly satisfied, nor would he adopt the mode, proposed by the late Captain Drummond, of measuring with broad bands, or ribands formed of mica; but feeling a preference for the principle of compensation, he gave it a new application, by inventing those admirable compensation bars, which stamped on the Irish Survey a character of novelty, and inseparably connected the name of their author with the history of geodetic science. It is not to be supposed that so great a work could have been carried on successfully without the cooperation of many most able and zealous officers; but when it is considered that their efforts were all directed and regulated on the system planned by General Colby, it must be felt that the Irish Survey, in its beginning and in its end, was eminently his own work.

Were this narrative now to end, it would fail to do full justice to the comprehensive mind of General Colby. When asked by Sir Henry Hardinge, then Clerk of the Ordnance, to state the advantages of a survey, he did not content himself by describing its ordinary usefulness, but nobly represented it as the proper basis for geological, statistical and antiquarian surveys. These views were acted upon at the commencement of the Survey, and to General Colby must therefore be ascribed the merit of having first originated a national Geological Survey, and, connected with it, a museum of Economic Geology. He did indeed more, as his scheme comprised natural history and antiquities; and the museum at Mountjoy contained not only a most valuable collection of minerals and fossils, but also an equally important one of the plants and animals of Ireland. It is true, that subsequently General Colby shrank from that responsibility which at first had seemed so light to him; that the Ordnance abandoned these collateral works; and that the Geological Survey passed into the hands of the Woods and Forests, there to acquire a full development under the able guidance of Sir Henry De la Beche; but let us recognize in the Memoir of Londonderry, published in 1835,—in the Report of the Geology of Londonderry and Tyrone, by Captain now Lieut.-Col. Portlock, R.E., published in 1843,—and in the Statistical Papers of the Census Commission drawn up by Major Larcom, and founded on the Statistical Section of the Memoir of Londonderry, to which that officer had so largely contributed, proofs that the scheme proposed by General Colby in 1824, would, if it had been followed up, have led to the publication of a national work, which, both in the grandeur of its conception and the importance of its results, would have been unrivalled by any such national work in Europe.

In 1825 Major Colby became Lieutenant-Colonel; in 1837 Colonel; and in 1846 Major-General. It is greatly to be regretted that his attainment of the last-named rank should have required that

his connexion with the great work he had so long and so ably directed, and with which his name will ever be most honourably associated, should cease.

General Colby was a Fellow of the Royal Society of Edinburgh; an Honorary Member of the Royal Irish Academy; a Fellow of the Geological, the Royal Astronomical, of the Geographical and Statistical Societies, and was also connected with the Society of Arts, either as Member or Proprietor, the Institution of Civil Engineers, and the Royal Institution. He received the degree of LL.D from the University of Aberdeen, and was a Knight of Denmark.

He died at Liverpool on the 9th of October 1852, in the sixty-ninth year of his age.

John Dalrymple was born at Norwich in the year 1804, and was the eldest son of the late William Dalrymple of that city, who although restricted to a provincial sphere, obtained a high reputation, and was known throughout Europe as one of the most successful operating surgeons in this country. The subject of our memoir entered the medical profession as pupil at the Norwich and Norfolk Hospital under his father, who was Surgeon to that Institution. He subsequently removed to the Borough Hospital, the schools of which were at that time united; and after completing his studies there, he became a member of the College of Surgeons in 1827, after which he commenced practice in the city. Mr. Dalrymple paid especial attention to the practice of Ophthalmic Surgery, and in the year 1832 was elected Assistant-Surgeon to the Royal Ophthalmic Hospital, where he contributed greatly, by his talent and high professional character, to raise that excellent charity to its present high standing in public estimation. During the period of his residence in the city he struggled against the disadvantage of almost continual ill health, to lay the foundation of a profound knowledge of the anatomy and diseases of that organ to which he had determined to devote his principal attention, and hence, on his removal to the west end of the town in 1839, he was well prepared for that great professional success which shortly after flowed in upon him; and from that period until his death, his onward course was only interrupted by the too frequent attacks of disease and his consequent general enfeebled health.

In 1843 Mr. Dalrymple was elected full surgeon to the Ophthalmic Hospital, and in the same year became a Fellow of the Royal College of Surgeons. In 1849 he was compelled by the state of his health to resign his appointment at the Ophthalmic Hospital, but the Governors of this charity, anxious to retain at least his occasional services, marked their sense of his high character and merits by appointing him Consulting Surgeon to the Institution.

Whilst, however, he was earnestly engaged in the honourable pursuit of professional fame, his hours of relaxation from that primary object had been constantly devoted to the pursuits of science, and we have had few more accurate and persevering investigators in microscopic anatomy and physiology than he was. To the most

acute observation and a rare dexterity of manipulation he added the happiest power of delineating the objects of his research, as an accomplished and accurate artist. In the year 1849, he presented to the Royal Society a paper on one of the most interesting subjects connected with the reproduction of animals, the discovery of the true male of the Rotifera, showing that this sex exists as a separate being, consisting, however, exclusively of the male organ, which is locomotive, but possesses no distinct alimentary apparatus. The animal which furnished the subject of Mr. Dalrymple's discovery was a species of Notommata, but since that time Mr. Gosse and other naturalists have observed the same remarkable fact in several other forms. This paper was published in the Philosophical Transactions for 1849.

Mr. Dalrymple did not however confine his scientific pursuits to the sciences of organic nature. He was one of the most strenuous promoters of that admirable and useful Institution, the Royal College of Chemistry, of which science he was a successful cultivator.

In 1850 Mr. Dalrymple was elected a Fellow of the Royal Society, and in 1851 was placed on the Council of the Royal College of Surgeons.

His great work on the pathology of the human eye, which had occupied his attention for many years, was only just completed when his useful and honourable career was prematurely cut short at the age of 48. Of this production it is no exaggeration to say that it is scarcely paralleled by any work on morbid anatomy which has ever appeared in this country. The masterly artistic beauty and accuracy of the illustrations are only equalled by the conciseness and practical importance of the descriptions.

Mr. Dalrymple was greatly endeared to his professional brethren and friends by the gentleness of his manners, the kindness and simplicity of his heart, and the nicest sense of professional honour.

Charles Morgan Elliot was born at Pimlico Lodge, Westminster, on the 27th of April, 1815; the ninth of fifteen children of the late John Elliot, Esq., F.R.S., his mother being the youngest daughter of the well-known Dr. Lettsom. Five of his brothers have been in the service of the East India Company; one of them, Sir Henry Elliot, K.C.B., is at this time Foreign Secretary to the Government of India.

Before ten years of age Charles was sent to Eton, which he left in less than three years, to prepare for Addiscombe, where he entered as cadet in 1830. At this Military Seminary he distinguished himself so much as to be appointed to the Engineers; and after passing the usual time at Chatham, he sailed for Madras in June 1833.

Early in 1838 his health required him to return to England; and during the two years he passed at home, he devoted himself assiduously to science; and was appointed Superintendent of the Magnetic Observatory at Singapore, at the same time that his brother officers Boileau and Ludlow were nominated to those of Simla and Madras.

He arrived at Singapore in 1840: and after remaining there five

years, laboriously employed in his scientific duties, he commenced in January 1846, the Magnetic Survey of the Eastern Archipelago. His observations were taken at sixteen different stations:—four in the islands adjacent to Singapore; one in Borneo; one in Java; two in Sumatra; one in the island of Mindanão; one in Celebes; one at the Cocos or Keeling Islands; one at Penang, and one in its immediate vicinity; one at Nicobar in the Bay of Bengal; one at Moulmein, and one at Madras. His zeal and energy will be appreciated, when we reflect that these fixed stations were spread over the immense area of 28° of latitude and 45° of longitude, and were carried on, under great privations, at great personal risk, and sometimes in places where no European had ever set foot before. In Borneo his fixed station was at Sarawak, near the house of his friend Rajah Brooke. Having completed his Survey at Madras in October 1849, he applied for furlough, and arrived in England at Christmas 1849, for the sole purpose of publishing his Observations —a work of great labour, which occupied him incessantly for nearly two years; they were printed in the Philosophical Transactions for 1851. Last December he returned to India in the hope of being able to carry through the magnificent undertaking of the Magnetic Survey of the whole Peninsula. In May last he left Madras, intending to go round by the coast through Masulipatam to Hydrabad. On his journey he was tempted to visit some extensive works carried on by the Government at Rajamundy, near the Godavery river, where he was seized with fever, and expired at Masulipatam, after a few days' illness, on the 4th of August last, at the early age of thirty-seven.

He was universally admired for the manliness of his character and beloved for his amiable social qualities.

He was elected a Fellow of the Royal Society in June 1850: the entire disinterestedness, self-sacrificing exposure in climates and seasons most unfriendly to life, and the well-directed ardour and remarkable ability (giving yet higher promise for the future had his life been prolonged) which marked his short but highly active scientific career, claim for his memory an honourable place in our records, and combined with the frankness, loyalty and sweetness of his character and temper, have endeared it in a peculiar manner to those amongst us who had most opportunity of appreciating him.

Gideon Algernon Mantell, LL.D., F.G.S. and F.L.S., was born at Lewes in 1790. His father was a shoemaker in the enjoyment, according to the statement of Mr. Thomas Mantell of Lewes, a brother of the subject of this memoir, of a large business, having as many as twenty-three men in his employ at one time. Dr. Mantell received his first instruction at a dame-school at Lewes, from which he was transferred to Mr. Button's establishment, also at Lewes, and subsequently was sent to a school in Wiltshire, conducted by a clergyman.

His father then articled him to Mr. James Moore, a surgeon and apothecary, paying a premium of two hundred guineas. Young

Mantell was fortunate in gaining the esteem of his master, who, after his pupil had 'walked the hospitals,' and what was then a novelty in country practice, become a licentiate of Apothecaries' Hall, admitted him into partnership, and he forthwith commenced practice, in which he was eminently successful. He made midwifery an especial study, and contributed several papers on that branch of medical science, and on the use of ergot of rye, to the 'Lancet,' and also other articles on various branches of medicine.

It is recorded greatly to his honour, and as a proof of his early attention to science, that with the assistance of his brother, the late Joshua Mantell, who was a surgeon at Newick, the life of a woman condemned to death for the murder of her husband by arsenic was saved, Dr. Mantell having distinctly proved that the tests used, and which were said to show the presence of this mineral poison, had entirely and chemically failed. This led to his publication, in 1827, of his 'Observations on the Medical Evidence necessary to prove the presence of Arsenic in the Human Body, in cases of supposed poisoning by that mineral.'

It was while Dr. Mantell was at Mr. Button's school that he first evinced a strong disposition for the study of natural history, and upon commencing practice at Lewes, he devoted as many hours as he could from his very arduous professional labours to the investigation of organic remains, first in the Chalk and next in the Tilgate formations, which were at that period comparatively new ground.

He was greatly encouraged in these researches by the late Mr. Davies Gilbert, and he was largely assisted by the zeal and knowledge of Mr. Stewart Warren Lee, who was his intimate friend and companion in all his early discoveries.

For nine years he devoted himself to the prosecution of his researches into the chalk formation, and in laying the foundation of the collection now in the British Museum. In May 1822 he published by subscription the result of his labours in a quarto volume, entitled 'The Fossils of the South Downs, or Illustrations of the Geology of Sussex.' The work was dedicated to Mr. Davies Gilbert.

In 1825 he communicated his first paper to the Royal Society, entitled 'Notice on the Iguanadon, a newly-discovered fossil reptile from the Sandstone of Tilgate Forest in Sussex.' This paper was printed in the Philosophical Transactions, and, at the recommendation of Mr. Davies Gilbert, its author was elected a Fellow of the Royal Society the year of its publication.

We now find him eagerly pursuing his favourite study of Geology. In 1826 he published his 'Illustrations of the Geology of Sussex,' with figures and descriptions of the fossils of Tilgate Forest, which include several discoveries that will always be associated with his name. He also contributed the Natural History of the district to Horsfield's 'History of Lewes,' and several articles on geology to different periodicals.

All this time he spared neither trouble nor money in collecting geological specimens for his museum, which soon became so famous, that parties from Brighton were in the habit of going to see it.

Indeed it is as a working geologist, as a discoverer, as a collector, and as one who in the infancy of geological science placed before the world the means by which others could write a thesis or found a system, that Dr. Mantell's merits were best displayed, and will be honestly acknowledged.

At the instigation of the Earl of Egremont, who was a warm friend of Dr. Mantell's, and evinced his admiration of his scientific labours by contributing the sum of £1000 to aid in the formation of his museum, the latter as well as Dr. Mantell's private establishment was removed to Brighton. This change was effected in 1835, but from some unexpected causes the high professional success which attended Dr. Mantell in his native town did not follow him to his new home.

In 1838 his patron, the Earl, died, and an attempt to keep the museum in Sussex, by the aid of local subscriptions, having failed, Dr. Mantell disposed of his collection to the British Museum for the sum of £5000, and shortly after he went to reside at Clapham, from whence he finally removed to Chester Square.

His professional practice was not increased by these removals, and was additionally injured by his great devotion to science and archæology; for he was a keen follower of the latter, and opened many tumuli near his native town. He also communicated a paper to the British Archæological Association 'On the connexion between Geology and Archæology.'

His removal to London did not damp his ardour for collecting remarkable geological specimens, and those who have had the gratification of attending the brilliant soirées of the late Marquis of Northampton, and those of the present distinguished President of the Royal Society, will remember how largely the subject of this memoir contributed, by the exhibition of numerous objects in geology and natural history, to the scientific enjoyment and instruction of the evening.

Indeed, although he was naturally proud of his acquisitions, which were often of a most remarkable character, he did not hoard them up, but was always ready and willing to allow geologists to use them for scientific purposes.

Among his latest contributions to palæontology, which science his labours have tended greatly to advance, may be particularly mentioned his paper on the *Dinornis*, an extinct bird of New Zealand, the bones of which extraordinary creature were sent to him by his son.

It is to him we are also indebted for the only specimen of the *Notornis*, also from New Zealand.

Dr. Mantell's works and writings are extremely numerous. He was a frequent contributor to the Transactions of the Geological Society, on whose Council he served for many years. He also acted as Hon. Secretary in 1841–42, and was Vice-President in 1848 and 1849. His communications to the Royal Society are as follows:—

Notice on the Iguanodon, a newly-discovered fossil reptile from the Sandstone of Tilgate Forest in Sussex. Read Feb. 10, 1825.

Memoir on a portion of the Lower Jaw of the Iguanodon, and on the remains of the Hylæosaurus and other Saurians, discovered in the Strata of Tilgate Forest in Sussex. Read Feb. 18, 1841.

On the Fossil remains of Turtles, discovered in the Chalk Formation of the South-east of England. Read May 20, 1841.

On the Fossil remains of the soft parts of Foraminifera, discovered in the Chalk and Flint of the South-east of England. Read June 18, 1846.

Observations on some Belemnites and other Fossil remains of Cephalopoda, discovered by Mr. R. N. Mantell, C.E., in the Oxford Clay near Trowbridge in Wiltshire. Read March 23, 1848.

On the Structure of the Jaws and Teeth of the Iguanodon. Read May 25, 1848.

Additional Observations on the Osteology of the Iguanodon and Hylæosaurus. Read March 8, 1849.

On a Dorsal dermal Spine of the Hylæosaurus, recently discovered in the Strata of Tilgate Forest. Read June 13, 1850.

Supplementary Observations on the Structure of the Belemnite and Belemnoteuthis. Read Feb. 14, 1850.

On the Pelorosaurus; an undescribed gigantic terrestrial reptile whose remains are associated with those of the Iguanodon and other Saurians in the Strata of Tilgate Forest. Read Feb. 14, 1850.

All these papers are printed in the Transactions. For those on the Iguanodon he received a Royal Medal in 1849.

The Bibliographia Zoologiæ et Geologiæ of the Ray Society contains the titles of sixty-seven books and essays from the pen of Dr. Mantell.

Among the more important of his works on Geology are the following:—

The Wonders of Geology, first published in 1838. It has passed through six editions, and has been translated into German.

The Geology of the South-east of England. 1838.

The Medals of Creation, 2 vols. 8vo, 1844. A recent edition of this instructive work has been published.

Thoughts on a Pebble. Seven Editions.

A Geological Excursion round the Isle of Wight, and along the adjacent Coast of Dorsetshire.

Petrifications and their Teaching. This was one of the last of the author's works, and was intended as an introduction to the organic remains in the British Museum.

As a lecturer as well as author, Dr. Mantell was eminently successful. His style was fluent, and he possessed the art of attracting his audience by an exhaustless catalogue of wonders.

No one who has enjoyed the advantage of hearing him can forget the singular ability, the felicitous illustrations, and the energetic eloquence that characterized all his discourses.

It is unhappily not the fate generally of the ardent pursuer of science, who is at the same time obliged to follow a laborious profession, to enjoy the *mens sana in corpore sano*. Dr. Mantell's life formed no exception to this rule, for his vigorous intellect was accompanied by an amount of bodily suffering which darkened many years of his life, and was eventually the indirect cause of his death.

This suffering proceeded from a spinal affection caused by an ac-

cident; but it is an additional proof of Dr. Mantell's great fortitude, that frequently at the cost of much self-denial, and the pressure of severe bodily pain, he made his appearance before a scientific society, or in a lecture-room, and it was under such painful circumstances that he lectured only a few hours before his decease.

This melancholy event was occasioned by his having prescribed opium for himself to relieve the agony which he was enduring, and which, although not sufficiently large to have produced fatal effects on a full stomach, proved in his exhausted condition so powerful as to induce death.

George Richardson Porter, Esq. was born on the 29th of June, 1790; he was brought up for mercantile pursuits, and commenced life as a wine-merchant in London. Being, however, unsuccessful in business, he turned his attention to literature, for which he was well qualified by his previous studies and pursuits; as it was his habit from earliest youth to compose (though not publish) papers on any subject which interested him. His first published work was the 'History of the Sugar-Cane' (in 1830). This book, together with other circumstances, led to an introduction to Mr. Charles Knight, who immediately gave him literary occupation, and the acquaintance turned out to be highly advantageous to the author.

Mr. Porter wrote several papers for the Companion to the Almanac, &c., and was for some years a constant and valuable contributor to the Penny Cyclopædia. But Mr. Knight's just appreciation of his abilities produced to him much greater and more lasting advantages than casual employment for his pen. Mr. Knight having been asked by the late Lord Auckland, when his Lordship was President of the Board of Trade, to undertake the task of arranging and digesting for the Board the mass of information contained in Official Books and Parliamentary Returns, Mr. Knight felt that he could not enter upon the work without injuring his publishing business, and he declined it, but he at the same time strongly recommended Mr. Porter to Lord Auckland as a person highly qualified for the undertaking.

This was in the year 1832, at which period the department of statistics at the Board of Trade was first organized as an experiment; but at the end of two years the utility of the department was so evident that it was definitely established, and Mr. Porter was placed at its head as Superintendent. It was here that he had access to those stores of information which his peculiarly statistical turn of mind enabled him to calculate and arrange with so beneficial an effect for public use, and few official volumes have tended more to introduce important commercial reforms than that which emanated yearly from the Statistical Department of the Board of Trade under the laborious and careful editorship of Mr. Porter.

In 1840 he was appointed senior member of the Railway Department of the Board of Trade. In the transaction of the arduous duties of that department, which, in 1845, when railway speculation was at its height, increased to an overwhelming extent, Mr. Porter's

services were as valuable as they were energetic, and were thoroughly appreciated by Lord Dalhousie, who then so efficiently presided over the department. On the retirement of Mr. McGregor in 1847, Mr. Porter was appointed one of the Joint Secretaries to the Board of Trade. This promotion added greatly to Mr. Porter's labours.

And yet, though an incessant worker in his office, he afforded another exemplification that the busiest man has often the most leisure, for it was while occupied by official duties, whose magnitude would have alarmed many men, that he found time, without in any way neglecting those duties, to write his 'Progress of the Nation,' which has passed through several editions, and which will be of incalculable value to future political economists. The amount of information in this very remarkable work, and the manner in which it is presented to the reader, entitles Mr. Porter to take the highest rank in the science of political economy.

Mr. Porter was the author of various other works in Statistics and Political Economy, and he wrote the 15th and last Section of the Admiralty Manual of Scientific Inquiry.

His contributions to the Statistical Section of the British Association for the Advancement of Science were very numerous and valuable, and he made frequent communications to the Statistical Society, which are printed in the Society's Journal.

Mr. Porter was one of the earliest promoters of that Society, and was chosen its Treasurer in the place of Mr. Hallam, who resigned that office in 1841.

His scientific labours admitted him to the Fellowship of the Royal Society, into which he was elected in 1838, and he was on the Council during the years 1847 and 1848. He was also a Corresponding Member of the Institute of France.

Mr. Porter's integrity, his elegant and varied accomplishments, and his amiable disposition, rendered him a cherished ornament of a large social circle, and he was always ready and willing to do all in his power to assist in any humane undertaking.

A remarkable instance of this disposition was communicated by Mr. Porter to the writer of this memoir, and which, as being connected with Sir Joseph Banks, when President of the Royal Society, is worthy of mention.

In consequence of the seizure by England of the Danish Fleet in the early part of this century, Iceland was afflicted by grievous famine, so that almost the only resource of the inhabitants for obtaining food was the sea-weed left by the receding tides. Under these circumstances a merchant from Copenhagen arrived in England with introductions to the mercantile house with which Mr. Porter was connected. His object was to obtain from the British Government licenses for the protection of Danish ships which should be employed in conveying provisions to Iceland; but his applications to the Board of Trade were, in the first instance, quite unsuccessful. As soon as Mr. Porter became aware of these facts, he remembered that Sir Joseph Banks, who had visited Iceland in 1772, was an Honorary

Member of the Board of Trade, and he determined to enlist, if possible, his sympathy in the cause of the unfortunate Icelanders. He wrote at length to Sir Joseph, and received an immediate answer, with the assurance that he had succeeded in securing his most zealous co-operation. On the same day that he received Mr. Porter's communication, Sir Joseph Banks went to the Board of Trade, and though at that period he was suffering greatly from bodily infirmity, he did not cease to employ his high interest, until, assisted by Mr. Porter, he had obtained the necessary licenses, which were immediately transmitted to Copenhagen.

Such conduct is equally honourable to the celebrated Baronet who so long filled the office of President of the Royal Society, and to the subject of this memoir.

It is to be feared that Mr. Porter's excessive anxiety to fulfil his arduous official duties led him to sacrifice his health when it needed repose and relaxation. His sedentary life proved the precursor of disease, which undermined his constitution, and after a short illness at Tunbridge Wells, to which place he had gone for his vacation, he died on the 3rd of September last. Mr. Porter was married to Sarah, daughter of Abraham Ricardo, Esq., and sister of Mr. David Ricardo.

Apart from his high private character, which was marked by a simple and unselfish integrity, which made him respected and loved by all within his influence, Mr. Porter will be long remembered as having a very remarkable power of quickly acquiring information on any given subject and making it completely his own; not merely compiling, but separating all the facts that were valuable out of the accumulated mass, and exhibiting them in a clear and succinct manner. This faculty also enabled him to condense an enormous amount of information in tabular forms.

Mr. Porter was a public servant of rare assiduity and zeal, and one whose qualifications for his important office were of the very highest order. The range of his commercial, statistical and political economy knowledge was of vast extent, and the readiness and precision with which he communicated it were extraordinary.

The Rev. JOHN WARREN, A.M. was born at the Deanery, at Bangor, in October 1796. He was the son of the Very Rev. John Warren, Dean of Bangor. He was educated at Westminster School and at Jesus College, Cambridge, of which he was Fellow and Tutor. In 1818, when he took the Degree of A.B., he was Fifth Wrangler. In 1825 and 1826 he served the office of Moderator and Examiner. He married his cousin Caroline Elizabeth, daughter of Lieut.-Col. Richard Warren. In 1830 he was elected a Fellow of the Royal Society. His death occurred at Bangor on the 16th of August, 1852.

In the year 1828 Mr. Warren published at Cambridge 'A Treatise on the Geometrical Representation of the Square Roots of Negative Quantities,' a subject which had previously attracted the attention of

Wallis* and of Heinrich Kühn†, Professor at Dantzig. The researches of these writers upon the geometrical representation of imaginary quantities were not known to Mr. Warren. A paper by M. Buée, containing some partially-developed views on the meaning and application of algebraic signs, was printed in the Philosophical Transactions for 1806, and a work by M. Mourey on the true Theory of Negative and Imaginary Quantities appeared at Paris in 1828. The former was unknown to Mr. Warren till his Treatise was in the press, and the latter was not seen by him before December 1828. It cannot therefore be said that he was indebted for his views to preceding writers; in fact, the work carries with it evident marks of originality, and has received honourable mention as well from Continental as from English mathematicians, who have since written on the same subject. The names of Buée, Warren, and Mourey are generally associated as having taken the lead in a department of mathematics, which in the present day‡ has received remarkable elucidations, developments and accessions at the hands of Gauss, Sir W. R. Hamilton, Professors Peacock, the late D. F. Gregory, De Morgan, C. Graves, and others.

The title of Mr. Warren's Treatise hardly conveys an exact idea of its main object. He proposes to represent every kind of quantity geometrically by the intervention of symbolical expressions, which involve the square roots of negative quantities, and designate lines in position as well as magnitude. After laying down definitions of addition, subtraction, multiplication, division, involution and evolution in the sense in which these operations must be taken when applied to quantity so represented, he proceeds to show the coincidence of the symbolical results obtained from such definitions with the ordinary results of arithmetical and symbolical algebra. He was strongly convinced of the superiority of geometry, as a means of demonstration, above the use of mere symbols of quantity, and entertained the opinion that the obscurity attaching to the proofs of some of the fundamental rules of algebraic and analytical operations, might be removed by adopting a geometrical representation of quantity, such as that proposed in his Treatise.

On Feb. 19, 1829, a paper by Mr. Warren, entitled "Consideration of the objections raised against the geometrical representation of the square roots of negative quantities," was read before the Royal

* 'Treatise of Algebra,' chapters lxvii.–lxix. fol. Oxford, 1685, cited by Ben. Gompertz, 'The Principles and Application of Imaginary Quantities,' book ii. 4to. London, 1818.

† *Commercium Mathematico-Petropolitanum*, anno 1736. *Meditationes de Quantitatibus Imaginariis construendis, et Radicibus Imaginariis exhibendis*, in *Nov. Comment. Acad. Scient. Imper. Petrop.* pp. 170–223, ad annos 1750 et 1751. Petrop. 1753.

‡ See George Peacock, 'Report on the recent Progress and present State of certain Branches of Analysis,' in 'Reports of the British Association for the Advancement of Science,' vol. iii. pp. 228–30. An account of several recent works upon this subject may be found in Wilhelm Matzka, *Versuch einer richtigen Lehre von der Realität der Vorgeblich imaginären Grossen der Algebra*, §§ 132–139, 4to Prag, 1850.

Society, and is printed in the Philosophical Transactions of that year. This was followed on June 4, 1829, by another paper, "On the geometrical representation of the powers of quantities, whose indices involve the square roots of negative quantities." Having previously confined his attention to representing geometrically quantities of the form $a+b\sqrt{-1}$, in the last memoir he succeeded in representing geometrically quantities of that form affected with an index of the same form $(a+b\sqrt{-1}\,m+n\sqrt{-1})$, and at the close of it stated that "it will be manifest from what has been demonstrated, that all algebraic quantity may be geometrically represented, both in length and direction, by lines drawn in a given plane from a given point." This extension of the subject Mourey had hinted at, but had not then published.

Mr. Warren's mathematical productions are limited to those above mentioned, with the exception of a short communication to the Cambridge Philosophical Society on a correction of Mourey's proof that every equation has as many roots as it has dimensions, which may be regarded as an instance of the scrupulousness with which he was accustomed to seek for exactness in mathematical demonstration. In the latter part of his life, when his time was chiefly taken up with ecclesiastical duties, he did not wholly lay aside mathematics, but with friends would converse largely on favourite topics, showing remarkable power in carrying on a mathematical demonstration *vivâ voce.* He continued to entertain a high estimate of the capabilities of the geometrical representation of impossible quantities, which he made some attempts to extend to space of three dimensions, and he even contemplated the possibility of applying it to the solution of the problem of three bodies. On these questions, however, he has left nothing in writing.

Mr. Warren was Chancellor of the Diocese of Bangor, and was the rector of Graveley in Cambridgeshire, and Caldecott in Huntingdonshire. He was also owner of the advowson of Caldecott, which, as well as an adjoining parish, was without a resident clergyman. In order to remedy this evil, he was desirous that the union of the two parishes might be effected. With this view he sold the advowson of Caldecott to the patron of the other parish, and gave up the purchase-money for the purpose of building a parsonage-house for the united parishes. It is not intended in this sketch to enter into details with respect to Mr. Warren's private history, but the above incident in his life is deemed worthy of record as being characteristic of the man. He was a clergyman of unaffected piety, simple habits and generous disposition.

On the motion of Sir R. H. Inglis, Bart., the best thanks of the Society were given to the President for his excellent Address, and his Lordship was requested to permit the same to be printed.

The Statutes relating to the election of Officers and Council having been read, and Sir C. Lemon, Bart., and Mr. W. Tooke having, with

the consent of the Society, been nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentlemen were reported duly elected Officers and Council for the ensuing year:—

President—The Earl of Rosse, K.P., M.A.

Treasurer—Colonel Edward Sabine, R.A.

Secretaries— { Samuel Hunter Christie, Esq., M.A.
Thomas Bell, Esq.

Foreign Secretary—Captain W. H. Smyth, R.N.

Other Members of the Council.—Rev. James Booth, LL.D.; Benjamin Collins Brodie, Esq.; Charles Brooke, Esq.; Lord Enniskillen, D.C.L.; J. P. Gassiot, Esq.; Thomas Graham, Esq., M.A.; Joseph Dalton Hooker, M.D.; William Hopkins, Esq., M.A.; Henry Bence Jones, M.D.; George Newport, Esq.; Lieut.-Colonel Portlock, R.E.; J. M. Rendel, Esq.; William Sharpey, M.D.; William Spence, Esq.; Nathaniel Wallich, M.D.; Lord Wrottesley.

Statement of the Receipts and Payments of the Royal Society between Dec. 1, 1851, *and Nov.* 30, 1852.

RECEIPTS.

				£	s.	d.
Balance in the hands of the Treasurer at the last Audit ..				147	8	6
Weekly Contributions, at one shilling				39	0	0
Quarterly Contributions at £4				1096	0	0
15 Admission Fees				150	0	0
4 Compositions for Annual Payments at £60				240	0	0
1 Composition for Annual Payments at £40				40	0	0
One year's rent of estate at Mablethorpe: due at Michaelmas 1851	116	16	0			
One year's Income Tax	3	8	0			
				113	8	0
One year's Fee farm rent of lands in Sussex: due at Michaelmas 1852				19	4	0
One year's rent from Royal College of Physicians				3	0	0
Carried forward........				£1848	0	6

				£	s.	d.
Brought forward..........				1848	0	6
Dividends on Stock:—						
One year's dividend on £14,000 Reduced 3 per cent. Annuities	420	0	0			
Less Income Tax	12	5	0			
				407	15	0
One year's dividend on £7591 4*s.* 2*d.* 3 per cent. Consols	227	18	11			
Less Income Tax	6	17	1			
				221	1	10
Half a year's dividend on £114 5*s.* 8*d.*	1	14	2			
Less Income Tax	0	0	11			
				1	13	3
One year's dividend on £3452 1*s.* 1*d.* 3 per cent. Consols, produce of sale of premises in Coleman Street.......................	103	11	2			
Less Income Tax	3	0	4			
				100	10	10
Donation Fund.						
One year's dividend on £5331 10*s.* 8*d.* Consols	159	18	6			
Less Income Tax	4	13	0			
				155	5	6
Rumford Fund.						
One year's dividend on £2430 12*s.* 5*d.* Consols	72	17	9			
Less Income Tax	2	1	9			
				70	16	0
Fairchild Fund.						
One year's dividend on £100 New South Sea Annuities				3	0	0
Bakerian Lecture and Copley Medal Fund.						
One year's dividend on £366 16*s.* 1*d.* New South Sea Annuities	10	18	0			
Less Income Tax	0	6	2			
				10	11	10
Wintringham Fund.						
One year's dividend on £1200 Consols	36	0	0			
Less Income Tax	1	1	0			
				34	19	0
Miscellaneous Receipts:—						
Sale of Philosophical Transactions, Abstracts of Papers, and Catalogues of the Royal Society's Library				304	11	0
One-half Expense of printing Captain Elliot's Magnetical Paper, repaid by the East India Company				255	0	4
Sale of One Rood and Thirty-Eight Perches of Acton Estate to Railway Company				115	0	0
Total Receipts......				£3528	5	1

PAYMENTS.

	£	s.	d.	£	s.	d.
Fairchild Lecture.—The Rev. J. J. Ellis, for delivering the Fairchild Lecture for 1852				3	0	0
Bakerian Lecture.—Professor Wheatstone, for the Bakerian Lecture for 1852				4	0	0
Croonian Lecture.—Professor Owen, for the Croonian Lecture for 1852				3	0	0
Salaries:—						
S. H. Christie, Esq., one year, as Secretary	105	0	0			
Thomas Bell, Esq., one year, as Secretary	105	0	0			
Ditto for Index to Phil. Trans.	5	5	0			
Capt. Smyth, one year, as Foreign Secretary	20	0	0			
Charles R. Weld, Esq., one year, as Assistant-Secretary	300	0	0			
Mr. White, one year, as Clerk	100	0	0			
Porter	40	0	0			
				675	5	0
Purchase of £114 5*s.* 8*d.* 3 per cent. Consols				115	0	0
Fire Insurance, on the Society's Property				45	1	6
Gratuity to Bank Clerks				1	1	0
Powers, Cleaning Rooms and Books				9	15	0
Repair of Instruments				20	8	7
Bills:—						
Taylor:						
Printing the Phil. Trans., 1851, part 2	259	8	6			
Ditto, 1852, part 1	102	0	6			
Ditto, Proceedings, Nos. 82—89; Circulars, Lists of Fellows, Ballot-lists, Statement of Payments, Minutes of Council; Government Grant Committee, Notices, &c. &c.	114	15	0			
				476	4	0
Basire:						
Engraving and Printing Plates in Transactions, 1852, part 1	74	19	10			
Ditto, part 2	177	19	2			
				252	19	0
Walker:						
For Engraving	254	10	9			
Gyde:						
For Wood Engraving	43	11	6			
Ford and West:						
Printing	66	0	0			
				364	2	3
Carried forward				£1969	16	4

	£	s.	d.	£	s.	d.
Brought forward..........				1969	16	4
Bowles and Gardiner:						
Paper for the Phil. Trans., 1851, part 2,	57	15	0			
and 1852, part 1...................	123	0	9			
				180	15	9
Gyde:						
Boarding and Sewing 800 Parts of Phil. Trans., 1851, part 2	22	18	0			
Ditto, 1852, part 1.................	11	4	0			
Ditto, Extra binding	25	9	9			
				59	11	9
Tuckett:						
Bookbinding	33	17	6			
Hyde:						
For Stationery	10	13	6			
Saunderson:						
For Shipping Expenses	14	3	9			
Brecknell and Turner:						
Candles, and Lamp Oil	17	18	2			
Arnold:						
For Coals	26	8	0			
Meredith:						
Mats, Brushes, Fire-wood, &c.	6	1	2			
Cubitt:						
For repairs and relaying Carpets, &c.......	32	14	7			
Hewitson:						
For Furniture.......................	15	16	0			
Ward:						
For Furniture.......................	15	15	0			
Charlton:						
For Cases and Shelves	6	19	0			
Humphries:						
For Livery	5	10	0			
Tea, Waiters, &c. at Ordinary Meetings	34	14	0			
				220	10	8
Books purchased:						
Dulau and Co.: for Books	37	2	9			
Taylor: for ditto	43	5	4			
Gould: for ditto	12	12	0			
Nutt: for ditto..	22	16	6			
Second-hand ditto	61	4	3			
				177	0	10
Taxes:						
Land and Assessed Taxes	18	12	9			
Income Tax	4	19	2			
				23	11	11
Carried forward..........				2631	7	3

				£	s.	d.
Brought forward				2631	7	3
Rumford Fund:						
Mr. Stokes, Medal and Dividends				141	12	0
Donation Fund:						
Mr. Cooper	15	0	0			
Balloon Committee of the British Association	261	2	5			
				276	2	5
Wintringham Fund:						
Governors of Foundling Hospital				198	16	3
Petty Charges:						
Postage and Carriage	40	13	5			
Expenses on Foreign Packets, &c.	4	3	3			
Stamps	0	7	6			
Charwoman's Wages	27	6	0			
Extra Cleaning	2	2	0			
Miscellaneous expenses	23	14	11			
				98	7	1
Balance in the hands of the Treasurer				182	0	1
Total				£3528	5	1

EDWARD SABINE, *Treasurer.*

November 30*th*, 1852.

Estates and Property of the Royal Society.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16*s*.
Estate at Acton, Middlesex (32 A. 2 R. 2 P.).
Fee farm rent in Sussex, £19 4*s*. per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per cent. Annuities.
£20,119 14*s*. 0*d*. Consolidated Bank Annuities.
£366 16*s*. 1*d*. New South Sea Annuities.

The Receipts during the past year, exclusive of the Balance of the last year and the Receipts from the Donation, Rumford, Fairchild, Wintringham, Bakerian and Copley Fund, were £3146 4*s*. 3*d*.

The Expenditure during the same period, exclusive of £115 0*s*. 0*d*. invested in the Funds, arising from the sale of land, and of sums paid on account of Trust Fund was £2604 14*s*. 4*d*.

Excess of Income over Expenditure £501 9*s*. 11*d*.

The augmentations to the Library included in the Expenditure of the present year, exclusive of binding, amount to £177 0*s*. 10*d*.

Cost of printing the Transactions, 1851, Part II. and 1852, Part I. £1218 17*s*. 9*d*., of which £255 0*s*. 4*d*. was repaid by the East India Company, leaving £964 7*s*. 6*d*. included in the expenditure of the present year.

Balance in hand belonging to the Wintringham Fund, £34 19*s*. 0*d*.

The following table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s.* Annually.	Paying £4 Annually.	Total.
December 1, 1851..	11	46	436	15	269	777
Since elected......	......	+4	+4	......	+11	+19
Since compounded	......	......	+1	......	−1	
Defaulters	......	......	......	......		
Withdrawn	......	......	......	......	−1	−1
Since deceased	......	−1	−21	......	−6	−28
November 30, 1852	11	49	420	15	272	767

Receipts by Annual Contributions.

1830	£363	4	0
1831	286	0	0
1832	255	6	0
1833	283	7	6
1834	318	18	6
1835	346	12	6
1836	495	0	0
1837	531	0	0
1838	599	4	0
1839	666	16	0
1840	767	4	0
1841	815	12	0
1842	910	8	0
1843	933	16	0
1844	1025	16	0
1845	1010	0	0
1846	1074	0	0
1847	1116	8	0
1848	1122	16	0
1849	1130	16	0
1850	1146	4	0
1851	1117	12	0
1852	1135	0	0

December 9, 1852.

COLONEL SABINE, R.A., Treas. V.P., in the Chair.

The Chairman announced that the President had appointed the following noblemen and gentlemen Vice-Presidents for the ensuing year:—

The Earl of Enniskillen.	Mr. Gassiot.
Lord Wrottesley.	Mr. Hopkins.
Col. Sabine, R.A.	Dr. Wallich.

Mr. Ward and Dr. Waller were admitted into the Society.

The following papers were read.

1. "An Experimental Inquiry undertaken with the view of ascertaining whether any signs of Current Electricity are manifested in Plants during vegetation." By H. F. Baxter, Esq. Communicated by Thomas Bell, Esq., Sec. R.S. &c. Received August 9, 1852.

In the present communication the author has related the experimental results that he has arrived at, and which tend to show that electric currents exist in the leaves and in the roots or spongioles of plants. Becquerel and Wartmann have already proved that electric currents may be obtained in different parts of vegetables, but the object of the paper is to point out the connection of the currents in the above-named organs with the vital or organic changes which take place in them.

In consequence of the secondary actions which occur at the electrodes some difficulty is experienced in ascertaining the true or normal result in the roots. Combining however the facts obtained by means of the galvanometer with analogical evidence, the author considers that they tend to establish the conclusion, that, during the changes which occur in the leaves and in the roots of plants, current electricity is manifested.

2. "On the relation of Cardioids to Ellipses." By Joseph Jopling, Esq. Communicated by S. H. Christie, Esq., Sec. R.S. Received Oct. 29, 1852.

The object of this communication is to point out the relation of cardioids to ellipses, and that the former as well as the latter are related to and deducible from the cone.

The author remarks that the motions of the common trammel show most beautifully the mechanical relation of ellipses and cardioids, and that they are thus reciprocals of each other; that an ellipse, as is well known, is a *plane section*, or a projection of a plane section of a cone upon any other plane, the limits being the circle and the right line; and a cardioid is also a projection from a cone; the difference being that the cardioid is obtained from a curved section, formed by the intersection of a sphere or other curved solid with a cone.

After referring to properties of the sections of cones by spheres, depending on the magnitude of the vertical angles of the cone, the author states that these and many other new curves, their relations,

and new properties of the cone and the sphere are made most clearly manifest, and numerous practical results are very readily obtained by the application of a double scale of sines to the rays of the cone—distributed equally on the plan—correspondingly on the elevation, and on the developed surface, or on any other projection of the cone.

He considers that it is of great importance that some method should be devised to give appropriate names to these new curves, especially those so evidently and intimately related to old ones. Thus the curved intersection of a cone and a sphere, from which the cardioid is projected on the base, and which has then the cusp turned symmetrically inwards, by another projection on a vertical plane gives a symmetrical line with the cusp turned outwards, having other distinct points of change in the curvature.

As ellipses are related to cardioids, so it is stated are hyperbolas in a similar way related to conchoids; conchoids to their mechanical reciprocals; and parabolas to cissoids; amongst the vast number of curves, any of which can conveniently be derived by scales practically from the cone.

By this method it is considered that not only projections of curved sections of cones, spheres, &c. are obtained with ease, but also by means of scales, ruled papers, hollow cones and diagrams, the plane sections of cones, and all projections from them are greatly facilitated.

In conclusion the author remarks, that if this subject can be entertained by the Royal Society, he will make copies of a series of diagrams to illustrate what he has here stated in reference to scales applied to cones to obtain cardioids, &c.

December 16, 1852.

J. P. GASSIOT, Esq., V.P., in the Chair.

A paper was read, entitled "On the Solution of Urinary Calculi in dilute Saline Fluids, at the temperature of the body, by the aid of Electricity." By H. Bence Jones, M.D., F.R.S., Physician to St. George's Hospital. Received Oct. 12, 1852.

In 1848 the author first attempted to dissolve calculi in a solution of nitrate of potash, by placing them at the same time between the electrodes of a galvanic battery; an effect was produced on the uric acid at the negative pole, but no very decided result was obtained. The experiments were resumed during the last summer.

The results with uric acid calculi may be thus arranged. The action was chiefly at the negative electrode.

Experiments.	Duration of experiment. h m	Strength and temperature of solution.		Power of Battery.	Result. grs.
1.	4	Saturated solution of nitre	212°	... 10 ...	0½ dissolved.
2.	6 5	¼ nitre, ¾ water	109	... 5 ...	11
3.	6 10	" "	101	... 10 ...	14
4.	6 20	" "	100	... 10 ...	16
5.	6 45	" "	106	... 10 ...	12
6.	3 17	" "	98	... 20 pair	27½

Oxalate of lime calculi were then tried.

Experiments.	Duration of experiment. h m	Strength and temperature of solution.	°	Power of Battery.	Result. grs.
1.	7	$\frac{1}{4}$ nitre, $\frac{3}{4}$ water	90	5	$0\frac{1}{2}$ dissolved.
2.	7	" "	104	10	2
3.	6 15	In sulphate of soda	101	10	2
4.	5 45	In common salt	102	10	1
5.	6 10	$\frac{1}{4}$ nitre, $\frac{3}{4}$ water	108	20	6
6.	3 19	$\frac{1}{4}$ nitre with phosphate of soda	110	20	1 dissolved.
7.	3 15	$\frac{1}{4}$ nitre with bichromate of potash	111	20	2
8.	3 17	$\frac{1}{2}$ nitre	110	20	$2\frac{1}{2}$
9.	2 50	$\frac{1}{4}$ nitre	92	20	$2\frac{1}{2}$
10.	3	"	100	40	5

Hence oxalate of lime calculi can be only very slowly dissolved in a solution of nitrate of potash, which acts far more energetically on uric acid calculi. The action is at least four times greater on uric acid calculi. Oxalates with urates, and oxalates with phosphates, were found to be far more rapidly acted on than oxalate of lime alone.

Phosphatic calculi were then taken. A piece of hard phosphate of lime gave

Experiments.	Duration of experiment. h m	Strength and temperature of solution.		Power of battery.	Result. grs.
1.	$7\frac{1}{4}$	$\frac{1}{4}$ nitre, $\frac{3}{4}$ water	102	10 pair	15
2. with fusible calculus	1 13	" "	96	20	31

The action was chiefly at the positive electrode.

Experiments with marble.

1.	$5\frac{1}{2}$	$\frac{1}{4}$ nitre	104	10	$27\frac{1}{2}$
2.	$6\frac{1}{2}$	In sulphate of soda	101	10	$4\frac{1}{2}$

From these experiments the following conclusions are drawn:— From two to nine grains of uric acid calculus can be dissolved in an hour in a neutral dilute solution of nitre at the temperature of the body, whilst in the same time from two to twenty-five grains of phosphatic calculus can be dissolved. However, only from half a grain to two grains of oxalate of lime can be removed by the same means in the same time. Still, if the stone consists of oxalate with urate, from one to two grains may be dissolved in an hour; and if it consists of oxalate with phosphate, from four and a half to five and a half grains can be taken away in that time.

These results may be obtained with calculi which have been long removed from the bladder, and have been dried at 212°.

MM. Prevost and Dumas in 1823 proposed to treat calculi by the galvanic pile. By means of the *mechanical action* of the mixed gases evolved by decomposing water, they found that phosphatic calculi might be disintegrated. The *chemical action* of the substances evolved by galvanic action on the surface of calculi was not determined by them.

The Society then adjourned over the Christmas vacation to the 6th of January, 1853.

January 6, 1853.

WILLIAM HOPKINS, Esq., V.P., in the Chair.

A paper was read, entitled "On Molecular Influences. Sect. I. Transmission of Heat through Organic Structures." By John Tyndall, F.R.S. Received Oct. 20, 1852.

In this paper the author has examined the influence exerted by the molecular structure of wood upon the passage of heat through the substance. Finding the usual modes of determining the conductivity of bodies inadequate to his purpose, he has been led to the construction of a new instrument which is capable of indicating very slight differences of transmissive power.

A cubical space is cut out of the centre of a rectangular slab of mahogany. The same slab holds a thermo-electric pair of bismuth and antimony, which are fixed in trenches cut out to receive them. The junction of the pair (which is of a V-shape) abuts upon one of the faces of the cubical space just mentioned; the end of a wooden slider forms the opposite boundary of the cubical space, and against this end a platinum wire, bent several times up and down so as to form a kind of micrometer-grating, is laid and imbedded in the wood. A small projection of ivory abuts at each side of the bismuth and antimony junction, and from one projection to the other a thin membrane is drawn, thus enclosing a space in front of the junction, which is filled with mercury. Two similar projections jut at the sides of the micrometer-grating, and across from one projection to the other, a second membrane is stretched, thus enclosing another chamber in front of the wire. This chamber is also filled with mercury, and against the wire a thin plate of mica is cemented, thus preventing all contact between the two metals. From the free ends of the bismuth and antimony bars wires proceed to a delicate galvanometer.

The substances to be examined by this instrument are reduced to the cubical form and placed between the two membranes; the slider being brought closely up against the cube, the latter is clasped firmly between the rigid projections before-named. The membranes are pressed gently against the two opposite faces of the cube by the mercury behind, and thus a contact is secured which, as the mercury is not changed during an entire series of experiments, remains perfectly constant. This is a most important point in experiments of this nature, for when the conditions of contact vary in even a slight degree, comparable results are out of the question. This remark of course applies exclusively to an inquiry like the present, where the object is to detect minute differences of molecular action. The protruding ends of the micrometer-wire are united to the poles of a small galvanic battery, and the wire is heated by the passage of the current; the heat is transmitted through the film of mica to the mass of mercury in front, which thus becomes the source of heat immediately applied to the face of the cube. The current is permitted to circulate through the bent wire for 60 seconds. During this time the heat passes from the face of the cube in contact with

the source to the opposite face; the quantity transmitted to the opposite face at the end of a minute, will of course depend on the conductivity of the body in the given direction. This quantity is measured by its effect upon the galvanometer.

The temperature of the source will, of course, depend upon the amount of electricity transmitted through the bent wire, and to preserve this amount perfectly constant from day to day, a tangent galvanometer and rheostat are introduced into the voltaic circuit; a current which produces the invariable deflection of 35° is made use of to heat the wire. By this arrangement experiments which are separated from each other by long intervals of time are rendered strictly comparable.

In the manner above indicated, the author has submitted fifty-four different kinds of wood, both English and foreign, to examination. The cubes were taken so that four faces of each were parallel to the fibre, and the remaining two consequently perpendicular to it. Of the four parallel to the fibre, two opposite ones were parallel to the ligneous layers, and the other two perpendicular to them. The amount of heat transmitted in 60 seconds across the mass of each cube in these three directions, respectively, was determined in the way described, and the following law of action established:—

At all points except the centre of the tree, wood possesses three unequal axes of calorific conduction which are at right angles to each other. The first and greatest axis is parallel to the fibre; the second axis is perpendicular to the fibre, and to the annual layers of the wood; while the third and least axis is perpendicular to the fibres and parallel to the layers. It is observed that these axes of calorific conduction coincide in order of magnitude and in direction with the axes of elasticity discovered by Savart.

The author furthermore points out the existence of two other systems of axes in wood,—the axes of cohesion and the axes of fluid permeability, both of which coincide with the axes of calorific conduction.

Experiments have been made on the conductivity of various other bodies, and the non-conducting powers of the substances which enter into the composition of organic tissues is strikingly exhibited. From comparative experiments with quartz and some other substances, the author points out the influence which a mass of silica exposed to the sun's rays, as in the African deserts, must exert upon climate.

The paper concludes with experiments on some other organic structures: Tooth of Walrus, Tooth of Elephant, Whalebone, Rhinoceros's-horn, Cow's-horn; and which show how small is their transmissive power: that of sealing-wax, bees'-wax, sole-leather, glue, gutta-percha, India-rubber, filbert-kernel, almond-kernel, boiled ham-muscle, raw veal-muscle, appears to be unappreciable by the method described.

January 13, 1853.

COLONEL SABINE, Treas., V.P., in the Chair.

The Earl Granville was elected a Fellow of the Society.

A paper was read, entitled "Description of some species of the extinct genus *Nesodon*." By Professor Owen, F.R.S. Received Nov. 25, 1852.

The author commences by referring to a genus of extinct herbivorous mammals which he had founded in 1836, on certain fossil remains discovered in Patagonia, and which, from the insular disposition of the enamel folds characteristic of the molar teeth, he had called *Nesodon*. Subsequent transmissions of fossils from the same part of South America, by their discoverer, Capt. Sulivan, R.N., now enabled the author to define four species of the genus. The first which he describes is founded on a considerable portion of the cranium and the lower jaw, with the teeth, and is called *Nesodon ovinus*. After the requisite osteological details and comparisons the author proceeds to describe the three incisors, the canine, and five molar teeth, which are present on each side of both upper and lower jaws, and then enters upon an inquiry as to the nature and homologies of the grinding teeth. The result is to show that the first four molars belong, with the incisors and canines, to the deciduous series, and that the fifth molar is the first true molar of the permanent series; the germ of a second true molar was discovered behind this, in both the upper and the lower jaws, whence the author concludes that the *Nesodon ovinus* had the typical number of teeth when the permanent series was fully developed, viz. $i\,\frac{3-3}{3-3}$, $c\,\frac{1-1}{1-1}$, $p\,\frac{4-4}{4-4}$ $m\,\frac{3-3}{3-3}=44$.

The structure of the grinding teeth proving the extinct animal to have been herbivorous, the number and kinds of teeth in the entire series show that it was ungulate. In this great natural series of mammalia the author next shows that the *Nesodon* had the nearest affinities to the odd-toed or perissodactyle order amongst the existing species; but certain modifications of structure, hitherto peculiar to the even-toed or Artiodactyle Ungulates, are repeated in the cranium of the *Nesodon*: more important marks of affinity are pointed out in the *Nesodon* to the *Toxodon*; and both these extinct forms of South American herbivores are shown to agree with each other in characters of greater value, derived from the osseous and dental systems, than any of those by which the *Nesodon* resembles either the Perissodactyle or Artiodactyle divisions of hoofed animals.

The genus *Nesodon* is characterized by the following modifications of the teeth, which in number and kind are according to the typical dental formula above given. *Incisors* trenchant, with long, slightly curved crowns, of limited growth: *canines* small, not exceeding in length the contiguous premolars. *Molars*, in the upper jaw, with long, curved, transversely compressed crowns, which contract as they penetrate the bone and ultimately develope fangs; the outer side of the crown ridged, the inner side penetrated by two more or less

complex folds of enamel, leaving insular patches on the worn crown: enamel thin. The *lower molars*, long, straight, and compressed; divided by an external longitudinal indent into two unequal lobes, both penetrated at the inner side by a fold of enamel, which is complex in the hinder lobe. All the teeth have exserted crowns of equal height and arranged in an unbroken series. The bony palate is entire and extends back beyond the molars, the maxillaries and palatines forming the back part in equal proportions. A distinct articular cavity and eminence for the lower jaw; the eminence long and concave transversely, short and convex longitudinally; a protuberant post-glenoid process; a strong and deep zygoma, the orbit and temporal fossa widely intercommunicating; the premaxillaries join the nasals.

Of the genus presenting the above dental and osteal characters the author defines four species:—the first, about the size of a Llama, is the *Nesodon imbricatus*; the second, of the size of a Zebra, is the *Nesodon Sulivani*; the species to which belong the portions of skull, with the teeth, described in the present memoir, did not exceed the size of a large sheep, and is termed the *Nesodon ovinus*; fourthly, a species of the size of a Rhinoceros, *Nesodon magnus*, is satisfactorily indicated by a grinder of the upper jaw. In conclusion, the author remarks, that the osteological characters defining the orders of hoofed quadrupeds, called *Proboscidia*, *Perissodactyla* and *Artiodactyla*, are associated with modifications of the soft parts of such importance, as not only to establish the principle of that ternary division of the great natural group of *Ungulata*, but to indicate that the known modifications of the skeleton of the extinct Toxodons and Nesodons of South America, in the degree in which they differ from the osteology of the already defined orders of *Ungulata*, must have been associated with concomitant modifications of other parts of their structure which would lead to their being placed in a distinct division, equal to the *Proboscidia*; and, like that order, to be more nearly allied to the *Perissodactyla* than the *Artiodactyla*. This new division of the *Ungulata* the author proposes to call *Toxodontia*, and he remarks that its dental and osteal characters, while they illustrate the close mutual affinities between the Nesodons and Toxodons, tend to dissipate much of the obscurity supposed to involve the true affinities of the *Toxodon*, and to reconcile the conflicting opinions as to the proper position of that genus in the mammalian class.

The paper is illustrated by twenty-three highly-finished drawings, by Dinkel, of the fossil bones and teeth of the different species of *Nesodon*.

January 20, 1853.

J. P. GASSIOT, Esq., V.P. in the Chair.

The following papers were read:—

1. "On the Extension of the value of the ratio of the Circum-

ference of a circle to its Diameter." By William Rutherford, Esq., F.R.A.S. Communicated by S. Hunter Christie, Esq., Sec. R.S. &c. Received November 17, 1852.

The author, referring to a former communication on this subject, published in the Phil. Trans. 1841, states that, in the value of π there given to 208 places of decimals, there exists, in the latter part of one of the terms of the series for determining the value of $\tan^{-1}\frac{1}{99}$, a transposition of the figures of a recurring decimal, which vitiates a considerable number of the figures in the latter part of the value. This error had been detected in consequence of Professor Schumacher having observed that in the value of π which had been given him by M. Dase, who had calculated it to 200 places, from the formula $\frac{\pi}{4}=\tan^{-1}\frac{1}{2}+\tan^{-1}\frac{1}{5}+\tan^{-1}\frac{1}{8}$, the figures from the 153rd to the 200th differed entirely from those given by the author. The accuracy of M. Dase's result was confirmed by a double computation of Dr. Clausen of Dorpat, who deduced the value of π to 250 places of decimals, both by Machin's formula

$$\frac{\pi}{4}=4\tan^{-1}\frac{1}{5}-\tan^{-1}\frac{1}{239},$$

and by the formula

$$\frac{\pi}{4}=2\tan^{-1}\frac{1}{3}+\tan^{-1}\frac{1}{7};$$

and the author's result was shown to differ from the correct value by the periodic decimal $\cdot\dot{3}\dot{6}$.

Having been informed by Mr. W. Shanks of Houghton-le-Spring, that he had pushed his computation of the value of π to the extent of 318 decimals, the author resolved to extend his operations to upwards of 400 decimals. As Mr. Shanks had employed Machin's formula, the author resolved to make use of the same. At his request Mr. Shanks resumed his calculations, and has not only verified the author's value of π to 440 places of decimals, but has carried his own to the extent of 530 places. The author states that the values of $\tan^{-1}\frac{1}{5}$ and $\tan^{-1}\frac{1}{239}$, as well as the value of π, which are here subjoined, have been obtained by the independent computations of Mr. Shanks and himself, and that they both feel confident that these values are correct in every figure as far as 440 decimals.

$\tan^{-1}\frac{1}{5}=$ ·19739 55598 49880 75837 00497 65194 79029 34475 85103 78785
21015 17688 94024 10339 69978 24378 57326 97828 03728 80441
12628 11807 36913 60104 45647 98867 94239 35574 75654 95216
30327 00522 10747 00156 45015 56006 12861 85526 63325 73186
92806 64589 68061 89528 40582 59311 24251 61329 73139 93397
11323 35378 21796 08417 66483 10525 47303 96657 25650 48887
81553 09384 29057 93116 95934 19285 18063 64919 69751 94017
08560 94952 73686 73738 50840 08123 67856 15800 93298 22514
02324 66755 49211 02670 45743 78815 47483 90799 7

$$\tan^{-1}\frac{1}{239} = \cdot00418\ 40760\ 02074\ 72386\ 45382\ 14959\ 28545\ 27410\ 48065\ 30763$$
$$19508\ 27019\ 61288\ 71817\ 78341\ 42289\ 32737\ 82605\ 81362\ 29094$$
$$54975\ 45066\ 64448\ 63756\ 05245\ 83947\ 89311\ 86505\ 89221\ 28833$$
$$09280\ 08462\ 71962\ 33077\ 33759\ 47634\ 60331\ 84734\ 14570\ 33198$$
$$60154\ 54814\ 80599\ 24498\ 30211\ 46039\ 12539\ 49527\ 60779\ 68815$$
$$58881\ 27339\ 78533\ 46518\ 04574\ 25481\ 35867\ 46447\ 51979\ 10232$$
$$83097\ 70020\ 64652\ 82763\ 46532\ 96910\ 48183\ 86543\ 56078\ 91959$$
$$14512\ 32220\ 94463\ 68627\ 66155\ 20831\ 67964\ 26465\ 74655\ 11032$$
$$51034\ 35262\ 82445\ 12693\ 55670\ 49968\ 44452\ 47904\ 3$$

$$\pi = 3\cdot14159\ 26535\ 89793\ 23846\ 26433\ 83279\ 50288\ 41971\ 69399\ 37510$$
$$58209\ 74944\ 59230\ 78164\ 06286\ 20899\ 86280\ 34825\ 34211\ 70679$$
$$82148\ 08651\ 32823\ 06647\ 09384\ 46095\ 50582\ 23172\ 53594\ 08128$$
$$48111\ 74502\ 84102\ 70193\ 85211\ 05559\ 64462\ 29489\ 54930\ 38196$$
$$44288\ 10975\ 66593\ 34461\ 28475\ 64823\ 37867\ 83165\ 27120\ 19091$$
$$45648\ 56692\ 34603\ 48610\ 45432\ 66482\ 13393\ 60726\ 02491\ 41273$$
$$72458\ 70066\ 06315\ 58817\ 48815\ 20920\ 96282\ 92540\ 91715\ 36436$$
$$78925\ 90360\ 01133\ 05305\ 48820\ 46652\ 13841\ 46951\ 94151\ 16094$$
$$33057\ 27036\ 57595\ 91953\ 09218\ 61173\ 81932\ 61179\ 3$$

Commencing at the 441st decimal place, Mr. Shanks' additional figures are as follow :—

$$\left(\tan^{-1}\frac{1}{5}\right)\ldots\ 78985\ 02007\ 52236\ 96837\ 96139\ 22783\ 54193\ 25572\ 23284\ 13846$$
$$47744\ 13529\ 09705\ 46512\ 24383\ 02697\ 56051\ 83775$$

$$\left(\tan^{-1}\frac{1}{239}\right)\ldots\ 33177\ 28393\ 07086\ 31401\ 93869\ 51950\ 37058\ 64107\ 70855\ 85540$$
$$45223\ 55388\ 14237\ 67708\ 36515\ 69182\ 52702\ 00228$$

$$(\pi)\ldots\ 31051\ 18548\ 07446\ 23799\ 62749\ 56735\ 18857\ 52724\ 89122\ 79381$$
$$83011\ 94912\ 98336\ 73362\ 44065\ 66430\ 86021\ 39488$$

In conclusion, the author states that Mr. Shanks has computed the value of the base of the Napierian system of logarithms as well as the values of the Napierian logarithms of 2, 3, 5 and 10, to the extent of 140 places of decimals.

2. "An Account of a Deep-sea Sounding in 7706 fathoms, in 36° 49′ South Latitude, and 37° 6′ West Longitude." By Captain Henry Mangles Denham, R.N., F.R.S. Communicated by Rear-Admiral Sir Francis Beaufort, K.C.B., F.R.S., Hydrographer. Received January 20, 1853.

This sounding was obtained on a calm day, October 30, 1852, in the course of the passage of H.M. ship Herald, from Rio de Janeiro to the Cape of Good Hope. The sounding-line was $\frac{1}{10}$th of an inch in diameter, laid into one length, and weighing, when dry, 1lb. for every hundred fathoms. Captain Denham received from Commodore M^cKeever of the United States Navy, commanding the Congress Frigate, a present of 15,000 fathoms of this line, 10,000 fathoms on one reel, and 5000 on another; and considers it to have been admirably adapted for the purpose for which it was made and to which it was applied. The plummet weighed 9lbs., and was 11·5 inches in length, and 1·7 inch in diameter. When 7706 fathoms had run off the reel the sea-bottom was reached. Captain Denham states that Lieut. Hutcheson and himself, in separate boats, with

their own hands, drew the plummet up 50 fathoms several times, and after it had renewed its descent, it stopped, on each occasion, abruptly at the original mark to a fathom, and would not take another turn off the reel. The velocity with which the line run out was as follows:—

	h	m	s
The first 1000 fathoms in	0	27	15
1000 to 2000 fathoms in	0	39	40
2000 to 3000 fathoms in	0	48	10
3000 to 4000 fathoms in	1	13	39
4000 to 5000 fathoms in	1	27	06
5000 to 6000 fathoms in	1	45	25
6000 to 7000 fathoms in	1	49	15
7000 to 7706 fathoms in	1	14	15
	9	24	45

The whole time therefore taken by the plummet in descending through 7706 fathoms, or nearly 7·7 geographical miles of 60 to the degree, was $9^h\ 24^m\ 45^s$. The highest summits of the Himalaya, Dhawalagiri and Kinchinginga, are little more than 28,000 feet, or 4·7 geographical miles above the sea. The sea-bottom has therefore depths greatly exceeding the elevation of the highest pinnacle above its surface.

The strength of the line tried before the sounding was found to be equal to bear 72lbs. in air. The 7706 fathoms which ran out weighed, when dry, 77lbs., exclusive of the plummet, 9lbs. Great care was taken in the endeavour to bring the plummet again to the surface to show the nature of the bottom, but, whilst carefully reeling in, the line broke at 140 fathoms below the water-line, carrying away a Six's thermometer which had been bent on at 3000 fathoms.

A paper was also in part read, entitled "On the Eclipses of Agathocles, Thales and Xerxes." By George B. Airy, Esq., F.R.S. &c., Astronomer Royal. Received December 15, 1852.

January 27, 1853.

The EARL OF ENNISKILLEN, V.P., in the Chair.

A letter was read giving an account of "An Explosive Meteorite." By Francis Higginson, Esq., R.N. Communicated by Thomas Bell, Esq., Sec. R.S. &c. Received December 23, 1852.

The writer states that his attention having been aroused by the highly electrical state of the atmosphere during a severe gale of wind, he proceeded along the beach in the vicinity of Dover, at 2 A.M. on the morning of Friday, the 17th of December 1852.

It had blown very hard during the night, the wind veering from West to W.S.W., in occasional heavy squalls of rain and sleet, accompanied at intervals by faint flashing scintillations, which at

first, being considered sheet lightning, were only noticed from their unusual colour, a deep and sombre red. At about $4^h\ 50^m$ A.M., however, these flashes constantly emerging from a dense, triangular and very remarkable cloud in the S.E., which perceptibly increased in size with great rapidity, he was induced to observe it with minute attention. At $4^h\ 55^m$ A.M., Greenwich mean time, the cloud had assumed the form of a right-angled triangle, its hypothenuse, or longest side, tending east and west. At this instant he first heard a singular and extraordinary hissing sound in the air, not unlike that of a passing shot, which, although at first not very loud, was yet clearly distinguishable above the howling of the gale. At 5 A.M. the cloud had nearly doubled its original size, advancing steadily from the S.E. in a N.W. direction, or from nearly dead to leeward, towards the wind's eye; whilst the scintillations spoken of were emitted with increased rapidity. He also then first perceived in the centre of the cloud, a dull, red, obscure nucleus, or fire-ball, apparently about half the diameter of the moon, having a tail five or six times that length, from which the flashes mentioned were sent forth, of surpassing brilliancy, as the meteor clearly descended with great velocity through the air, accompanied by a detonating, hurtling, hissing sound, impossible to describe, yet resembling that which precedes the shock of an earthquake. At three minutes past five o'clock A.M., the meteor having apparently spanned the Channel from S.E. to N.W., upon approaching the land—evidently throwing off portions of its substance as it passed through the atmosphere—the nucleus suddenly exploded with a report similar to a very heavy clap of thunder, giving out an intensely brilliant light, which rendered the minutest objects distinctly visible, although it rained violently and the sky was obscured by dark and threatening clouds. The dense body of the meteorite seemed to fall in the water about half a mile from the land, as indicated by a great volume of spray, which rose foaming in the distance.

February 3, 1853.

COLONEL SABINE, R.A., Treasurer, V.P., in the Chair.

The following letters were read:—

MONSIEUR LE COMTE, À Berlin, ce 30 Dec. 1852.

C'est avec la plus vive reconnaissance, je pourrais dire, avec le genre d'émotion que l'on éprouve lorsqu'on obtient un succès auquel on n'a pas cru devoir aspirer, que j'ai reçu la médaille Copley, le grand et noble prix que la Société Royale, sous le Présidence de Monsieur le Comte de Rosse, a daigné m'adjuger. Cette illustre Compagnie a voulu recompenser un zèle ardent pour les sciences, des travaux peu remarquables par leurs resultats, mais fortifiés, dans le cours d'une longue et laborieuse carrière, par la constance d'une courageuse assiduité. En vous suppliant, Monsieur le Cômte, de

vouloir bien être, auprès de la Société Royale l'interprète des sentimens de profond respect et de gratitude dont je suis pénétré, je dois ajouter aussi (comme dejà l'aura fait, en mon nom, M. le Chevalier Bunsen, mon noble et spirituel ami) que je suis heureux d'adresser ces lignes à celui pour lequel dans le *Cosmos* même j'ai osé deposer l'hommage de ma vive admiration.

Je suis avec la plus haute et respectueuse considération,
Monsieur le Comte,
Votre très-humble et très-obéissant serviteur,
LE BARON DE HUMBOLDT.

A M. Le Comte de Rosse, P.R.S.

GENTLEMEN, London, February 3, 1853.

By the kind indulgence of my co-executors, Mr. George Thornton and Mr. John Daniell, I am permitted to announce to you a most interesting bequest to the Royal Society, from my uncle the Rev. Charles Turnor, F.R.S., who was suddenly taken from us on the 12th ultimo, namely, a collection of drawings, memoirs, a few medals, and other articles enumerated in a list, illustrative of Sir Isaac Newton.

The well known property at Woolsthorpe in Lincolnshire, which formerly belonged to Sir Isaac Newton, was purchased by the ancestor of Mr. Turnor in or about the year 1730, four years after his death.

My father-in-law, the late Mr. Turnor, wrote what he could collect in his 'History of the Soke of Grantham,' and his younger brother, the Rev. Charles Turnor, continuing the subject, has at great expense devoted much time to this collection. I may therefore be excused in saying that it is from no common hand you receive this valuable addition to your Library.

We send you a copy of the Codicil of the Will relating to the bequest, and will immediately arrange the several articles in the collection for delivery to you. I should certainly wish the Dial presented by Mr. Charles Turnor, to either form part of the collection, or be placed near it, as it was taken from the wall of Sir Isaac's residence at Woolsthorpe.

I am, Gentlemen,
Your obedient and humble Servant,
FREDERICK MANNING.

To the Secretaries of the Royal Society.

Extract from the Codicil to the Will of the Rev. Charles Turnor.

"I give and bequeath to the President and Council of the Royal Society at Somerset House, Strand, London, the sum of two hundred pounds free of legacy duty, in trust, that they shall apply and expend the same, at their discretion, in the completion of my collection called the 'Collectanea Newtoniana,' within the period of twelve months after my decease. And I hereby give and bequeath all the materials of which the said 'Collectanea' is composed, to the same President and Fellows of the Royal Society absolutely, to be

kept for ever in their apartments in Somerset House. And I hereby declare that my will and intention as to the arrangement of the memoirs, engravings, drawings, autographs, books, the gold watch formerly belonging to Sir Isaac Newton, the gems and other relics of which the said 'Collectanea' is composed, is expressed in a manuscript catalogue, which will be found in the box containing the collection, and signed by me. But it is not my intention that the plan therein described should be strictly adhered to, if the President and Council prefer any other method with regard to the arrangement of the work.

"And I hereby direct my executors to pay the above legacy within three months after my decease; and if two hundred pounds should not be sufficient for the purposes before mentioned, then my executors are hereby required and empowered to advance fifty pounds more (free of legacy duty); and if there should be any overplus in either case, then the President and Council of the said Society are at liberty to apply such overplus in the purchase of books for the Library, or in any other way for the benefit of the Royal Society."

The reading of Mr. Airy's paper, entitled "On the Eclipses of Agathocles, Thales and Xerxes," was resumed and concluded.

The author, after remarking that the calculations of distant eclipses made in the last century possess little value, proceeds to give the successive steps of improvement in the lunar theory as applicable to the computation of eclipses, and especially in the motion of the moon's node. The first great improvement was the introduction by Laplace of terms expressing a progressive change in the mean secular motions. With Bürg's tables, in which these changes were introduced, or with the same elements, Mr. Francis Baily and Mr. Ottmanns computed many eclipses in the search for that usually called the eclipse of Thales; and both these astronomers fixed upon the eclipse of B.C. 610, September 30, as the only one which could be reconciled with the account of Herodotus. Mr. Baily however subjoined a computation of the eclipse of Agathocles from the same elements, and found that this could not by any means be reconciled with the historical account; he inferred from this that some serious change in the theory is necessary, and that when it was introduced the eclipse of B.C. 610 might not be found to agree with history; but he thought it certain that no other eclipse could be adopted. The various values of the motion of the node adopted by different writers from different observations (principally total or annular eclipses) are then collected. Allusion is then made to the peculiar value of the eclipse of Stiklastad (brought to notice by Professor Hansteen), and which will be increased when the calculations shall have been made on unexceptional elements. The author then adverts to the great Reduction of the Greenwich Observations from 1750 to 1830, to Hansen's new inequalities, and to the numerical amounts of corrections of the principal elements. Then are given the coefficients of the change in secular value of mean motion of the mean of the moon's perigee, and of the moon's node, as found by

Laplace, Damoiseau, Plana, and Hansen; the principal change made by the latter writers from Laplace's values being in the motion of perigee.

The method of computation adopted by the author is then explained. He adopts the Greenwich mean motions and Damoiseau's coefficients for progressive change of secular mean motion. He then repeats the calculation with an arbitrary change of longitude of node; considering that, from the loose nature of the early Greenwich observations, this element is most likely to be in error, and that its errors will produce the greatest effect.

The author then discusses the account of the eclipse of Agathocles, B.C. 310, August 15. Adopting Alhowareah (under Cape Bon) as his landing-place in Africa, he states his belief that Agathocles sailed northward from Syracuse (a conjecture which he owes in the first instance to J. W. Bosanquet, Esq.), and was not far from the Straits of Messina. On the usual supposition of his sailing to the south, he would be near Cape Passaro.

On making the calculation with the Greenwich Elements unvaried, it is found that the eclipse would be total on the southern possible place of Agathocles, but not on the northern. The calculation being repeated with an arbitrary change in the place of the node, a graphical construction is employed to discover the numerical amount of the changes that must be made to satisfy the four following conditions:—1, the northern edge of the shadow touches the south station; 2, the northern edge touches the north station; 3, the southern edge touches the south station; 4, the southern edge touches the north station. If the south station be adopted, the change must lie between those of conditions 1 and 3; if the north, the change must lie between those of 2 and 4. The numerical values must be slightly increased for application to a more distant eclipse, as to that of Thales.

The eclipse of Thales is then considered. There appears to be no reason for connecting the locality (as Mr. Baily supposed) with the river Halys. The historical circumstances indicate with great probability that two large armies had met; and the question appears to be, in what part of Asia Minor is it likely that such bodies of troops would be collected. The author adverts to the form and passes of the mountains, and decides that the Median army entered most probably by the pass of Issus, or possibly by that of Melitene, and that the battle-field might be anywhere in the space bounded by Melitene, Issus, Iconium, Sardes, and Ancyra.

On calculating the eclipses which occurred for many years before and after B.C. 600, it appears that only the eclipse of B.C. 585, May 28, answers to the requisite conditions, and that it does so in a most satisfactory way. [This is the date adopted by the principal ancient chronologists; it would seem that it was first verified, by calculations founded on good elements, by J. R. Hind, Esq. during the preparation of this paper.] The path of the shadow which is most agreeable to the military and geographical circumstances, is one which implies a correction to the Greenwich Elements corresponding to that

which would make the eclipse of Agathocles nearly central over the northern station, and excludes the possibility of his passing by the southern route.

The author then adverts to the principal remaining causes of uncertainty in these conclusions, and points out the values of progressive change in the secular mean motions as peculiarly deserving investigation.

Allusion is then made to a record in the Persian poetical history, preserved by Sir John Malcolm, which appears to point to a total eclipse as occurring under similar circumstances in the province of Mazenderan. It appears however on calculation, that no total eclipse passed over Mazenderan, at least for many years, about the time in question.

The author then calls attention to the statement of Herodotus, that something like a total solar eclipse occurred when Xerxes was setting out from Sardes for his invasion of Greece. On calculation it appears impossible to explain this by a solar eclipse, and moreover the peculiar turn of the answer of the Magi to the inquiries of Xerxes would seem to be irreconcileable with a solar eclipse. The author thinks it most likely that the phenomenon really was the total eclipse of the moon which occurred on the morning of B.C. 479, March 14. If this were adopted, the date of the invasion of Greece must be brought down one year later than that given by the received chronology.

February 10, 1853.

LORD WROTTESLEY, V.P., in the Chair.

The following papers were read:—

1. "On the determination of the Mean Temperature of every day in the year, as deduced from the Observations taken at the Royal Observatory, Greenwich, in the Years from 1814 to 1851." By James Glaisher, Esq., F.R.S. Received Dec. 30, 1852.

This paper has for its object the determination of the true distribution of heat over the year, and is based upon an extensive series of observations taken at the Royal Observatory during thirty-eight years.

In order to obtain a correct determination of the mean daily temperature of each month, necessary to the proposed object, the author at the commencement of his memoir explains how the entire series of observations has been divided into groups, according to the recorded times of observation, for the purpose of applying the necessary corrections calculated from his tables of Diurnal Range, published in the Phil. Trans. for 1848. Having carefully explained his method of arranging and testing his data, and providing for exceptional days, upon which but few observations were recorded, the author gives the results in twelve separate tables, which exhibit the mean daily temperatures of every month in each of the thirty-eight years. In a

note to the table for each month are given:—1. The mean temperature of the coldest day of that month, with the day of the month and the year, from 1814 to 1851; 2. the mean temperature of the hottest day of that month, with the day of the month and year, and the extreme difference of mean temperature of two days in that mouth; 3. The day of the month on which the mean temperature was subjected to the greatest change, with the minimum and maximum mean temperatures, the year of the minimum and of the maximum; 4. the day of the month on which the mean temperature was subjected to the least change, with the minimum and maximum mean temperatures, the year of the minimum and of the maximum. These results are embodied in the opposite table:—

The author then treats of the method adopted to deduce the most probable true mean temperature due to every day in the year; and concludes his paper by observing that there are periods of some duration which are very remarkable on account of the difficulty of assigning a physical cause for the anomalies apparent in the mean temperature. Starting from the lowest temperature, in January, it increases till the beginning of March, when, between the 3rd and 10th, not only is the increase checked, but there is a remarkable depression of temperature. After the 10th, the increase proceeds and is very rapid towards the end of April and the beginning of May; this rapid increase is rather suddenly checked, and followed by a period of cold towards the middle of May: this period is very marked. As remarkable a depression of temperature at this time of the year seems to have taken place in France, having been noted in Paris and at various localities, some situated near the coast; but it does not appear that the equally remarkable rise at the end of April has been noted. After the middle of May the numbers steadily increase till the 5th of July, when they attain their maximum value. The decline in the temperature towards the end of July is followed by an increase at the beginning of August, after which the decline of temperature is very regular till towards the end of November, when a sudden and considerable increase of temperature takes place; after this the curve declines to its lowest point on the 8th of January.

	Mean temperature of coldest day from 1814 to 1851.	Day of month.	Year.	Mean temperature of hottest day.	Day of month.	Year.	Extreme difference of mean temp. of two days in the month.	Day of month on which mean temp. subjected to greatest change.	Minimum mean temperature.	Year.	Maximum mean temperature.	Year.	Difference.	Day of month on which mean temp. subjected to least change.	Minimum mean temperature.	Year.	Maximum mean temperature.	Year.	Difference.
January	10°·7	20	1838	52°·7	24	1834	42°·0	20	10°·7	1838	48°·0	1828	37°·3	6	26°·1	1841	46°·7	1844	20°·6
February	12·6	9	1816	55·0	9	1831	42·4	9	12·6	1816	55·0	1831	42·4	20	26·2	1845	48·0	1850	21·8
March	22·1	13	1845	58·6	31	1815	36·5	16	25·2	1845	54·3	1828	29·1	29	36·9	1816	53·8	1830	16·9
April	27·8	1	1836	63·2	25 26	1821	35·4	3 4	28·9 29·5	1839	60·7 61·3	1848	31·8	22	38·0	1837	55·9	1826	17·9
May	36·2	3	1832	72·4	15	1833	36·2	15	42·2	1839	72·4	1833	30·2	2	44·4	1850	59·8	1838	15·4
June	45·0	7	1814	76·1	13	1818	31·1	25	45·1	1835	71·5	1820	26·4	16	52·6	1850	69·4	1846	16·8
July	47·7	20	1836	79·1	15	1825	31·4	18	52·3	1816	78·2	1825	25·9	10	54·4	1835	67·8	1845	13·4
August	43·2	31	1833	75·3	1	1825	32·1	20	47·0	1839	73·3	1826	26·3	9	56·4	1848	68·9	1842	12·5
September......	40·7	28	1824	73·5	2	1824	32·8	1	44·5	1816	71·7	1824	27·2	23	47·1	1845	60·9	1846	13·8
October	28·4	29	1836	64·5	5	1834	36·1	29	28·4	1836	55·5	1847	27·1	4	45·0	1836	61·7	1837	16·7
November	23·4	24	1836	59·7	2	1834	36·3	24	23·4	1816	53·9	1846	30·5	4	35·1	1820	52·2	1834	17·1
December	18·4	24	1830	54·9	8	1848	36·5	25	18·6	1830	53·1	1824	34·5	4	31·1	1814	50·8	1836	19·7

2. "On the periodic and non-periodic variations of Temperature at Toronto in Canada from 1841 to 1852 inclusive." By Colonel Edward Sabine, R.A., Treasurer and Vice-President of the Royal Society. Received 20th Jan. 1853.

The principal object of this communication is to make known the non-periodic variations of temperature for every day in the twelve years, from 1841 to 1852 inclusive, at Toronto in Canada. The non-periodic variations are those differences of the temperature from its mean or normal state which remain after all the known periodical variations are allowed for, and are such as are generally accompanied by peculiarities of wind or of other meteorological circumstances. Recent investigations have led to the inference that opposite conditions of weather prevail simultaneously in the same parallels of latitude under different meridians, and that in particular Europe and America usually present such an opposition, so that a severe winter here corresponds to a mild one there, and *vice versâ*; and recent theories of the distribution of heat on the surface of the globe profess to furnish the explanation. To place the facts on indisputable ground, it is requisite that a comparison should be made of unexceptionable records of the non-periodic variations in Europe and America, continued for a sufficient time to afford a proper basis for inductive generalisation. Toronto, from its latitude 43° 40′ N. and inland situation, is well suited to supply such a comparison with stations in the middle parts of Europe where similar records have been kept; and the twelve years embraced by the observations, viz. 1841 to 1852, have been years of unusual meteorological activity in Europe.

Details are given in the commencement of the paper showing the care bestowed on the examination of the thermometer employed, with a standard divided "*à l'échelle arbitraire*," by the method of M. Regnault; as well as the precautions adopted for its fair exposure, and for its protection from rain and radiation. The observations were made by the non-commissioned officers of the detachment of the Royal Artillery employed in the duties of the observatory.

The period of twelve years comprises two series, in one of which the thermometer was observed hourly, and in the other less frequently, each observation in the second series receiving however a correction to the mean temperature of the day furnished for every hour and every day of the year by the first series. The two series, each of six years, are separately discussed; from the first series equations are derived from the mean monthly temperatures by the method suggested by Bessel (Astron. Nach. No. 136), whereby the most probable values of the temperature, on every day and every hour, are computed corresponding to the whole body of the observations. These the author regards as approximate normal values, and by comparing with them the actual daily temperatures,—which in the first six years are the means on each day of twenty-four equidistant observations, and in the second six years the means of all the observations made on each day, each observation having been corrected

for the hour in the manner described,—the non-periodic variations for every day in the year are obtained and are given in a table.

From the approximate normal temperatures the author has represented in a Plate the phenomena of the temperature at Toronto, according to a method which, if applied to the different meteorological elements and in different localities, might, he thinks, materially facilitate their intercomparison. This method, in which three variables are represented, one being dependent on the other two, is essentially the same that has been long used in magnetic maps, and in the ordinary isothermal maps; from which latter however it differs in this respect, that, whereas in the ordinary isothermal maps the two variables on which the variation of temperature is dependent are the geographical latitude and longitude, in the present case the two variables are the hour of the day and the day of the year. The variation of temperature is here referred therefore to *time* and not to *space*; a distinction which the author proposes to convey by employing the term Chrono-Isothermals, as applicable to lines of this description. From the delineation in the Plate, and from the tables contained in the paper, many characteristic and some peculiar features of the climate and meteorology of the part of the North American Continent in which Toronto is situated, are readily perceivable. Several instances are pointed out; amongst these may be noticed the peculiar anomaly of the North American winter, which is very conspicuous in the Plate; and the absolute as well as relative *variability* of the temperature at different seasons of the year, exhibited by means of a numerical index analogous to the probable error of the arithmetical mean of a number of partial results, and deduced in a similar manner from the differences of individual years, months, and days, from their mean values: whence it appears, in respect to the annual temperature, for example, that in any particular year there is an equal probability that its mean temperature will fall within the limits of $43°{\cdot}6$ and $44°{\cdot}9$, as that it will exceed those limits on either side.

Finally, the author has shown the "Thermic Anomaly" (as it has been recently termed) of the monthly and annual temperatures at Toronto by comparison with the normal temperatures computed by Dove (Verbreitung der Wärme, 1852), for the parallel of 43° 40′ N. from 36 equidistant points on the parallel; from which comparison it appears that after allowance has been made for the elevation above the sea (342 feet), every month of the year is colder than the normal temperature of the same month in the same parallel; that the thermic anomaly reaches its extreme in February, when it exceeds 10° of Fahrenheit; and that on the average of the whole year it is little less than 6°.

February 17, 1853.

J. P. GASSIOT, Esq., V.P., in the Chair.

A paper was read, entitled "On the Muscles which open the Eustachian Tube." By Joseph Toynbee, M.D., F.R.S. Received February 2, 1853.

The author commences by alluding to the opinion generally held by anatomists, viz. that the guttural orifice of the Eustachian tube is always open, and that the air in the tympanum is constantly continuous with that in the cavity of the fauces. An examination of the guttural orifice of the tube in man and other animals has led the author to conclude, that, except during muscular action, this orifice is always closed, and that the tympanum forms a cavity distinct and isolated from the outer air. The muscles which open the Eustachian tube in man are the tensor and levator palati, and it is by their action during the process of deglutition that the tubes are ordinarily opened. That the act of swallowing is the means whereby the Eustachian tubes are opened, is shown by some experiments of which the following may be cited. If the mouth and nose be closed during the act of swallowing the saliva, a sensation of fulness or distension is produced in the ears; this sensation arises from the air, which is slightly compressed in the fauces, passing into and distending the tympanic cavities: upon removing the hand from the nose, it will be observed that this feeling of pressure in the ears does not disappear, but it remains until the act of deglutition is again performed while the nose is not closed. In this experiment the Eustachian tubes were opened during each act of deglutition; during the first act, while they were open, air was forced into the cavity of the tympanum by the contraction of the muscles of the fauces and pharynx, and the guttural orifices of the tubes remained closed until the second act of swallowing, which opened the tubes and allowed the air to escape. That the act of deglutition opens the Eustachian tubes, was inferred also from the custom usually adopted of swallowing while the descent in a diving-bell is performed; by this act the condensed air is allowed to enter the tympanum, and the sensation of pain and pressure in the ears is removed or entirely avoided.

The author gives an account of the Eustachian tube and its muscles in Mammalia, Birds and Reptiles. In some mammalia the muscles opening the tubes appertain, as in man, to the palate, in others this function is performed by the superior constrictor muscles of the pharynx. In Birds it is shown that there is a single membranous tube into which the two osseous tubes open; this membranous tube is situated between and is intimately adherent to the inner surface of each pterygoid muscle, and by these muscles the tube is opened. The conclusion to which the author arrives respecting the influence of the closed Eustachian tubes is, that the function of hearing is best carried on while the tympanum is a closed cavity, and that the analogy usually cited as existing between the ordinary musical instrument, the drum and the tympanum, to the effect, that in each it is requisite for the air within to communicate

freely with the outer air, is not correct. On the contrary, the author shows that no displacement of the air is requisite for the propagation of sonorous undulations, and that were the Eustachian tubes constantly open, these undulations would extend into the cavity of the fauces, there to be absorbed by the thick and soft mucous membrane, instead of being confined to the tympanic cavity, the walls of which are so peculiarly well adapted to the production of resonance, in order that they shall be concentrated upon the labyrinth.

In corroboration of the above views the author states, that in cases of deafness dependent simply upon an aperture in the membrana tympani, whereby the sonorous undulations are permitted to escape into the external meatus, the power of hearing has been greatly improved by the use of an artificial membrana tympani made of vulcanized india-rubber or gutta-percha, which is so applied as again to render the tympanum a closed cavity.

February 24, 1853.

COLONEL SABINE, R.A., Treas. & V.P., in the Chair.

A paper was read, entitled "On Periodical Laws in the larger Magnetic Disturbances." By Captain Younghusband, R.A., F.R.S. Received February 16, 1853.

In this communication the author has arranged, in tables, the disturbances of the magnetic declination at St. Helena and the Cape of Good Hope, for the purpose of exhibiting the systematic laws by which those phenomena are regulated, which were long described as irregular variations, because they were of occasional and apparently uncertain occurrence.

The frequency of the disturbances, and their amount, whether viewed separately as easterly or westerly movements, or as general abnormal variations (easterly and westerly being taken together), is shown to be dependent upon the hour of the day, the period of the year, and upon the year of observation. This dependence upon the year of observation affords additional testimony of a periodical variation in the magnitude of magnetic changes of the same character as that which has been found to exist at other places, and which has been considered to be coincident with variations of the solar spots.

The disturbances of larger amount only are noticed; those observations which differed by 2·5 scale divisions (1'·8 in arc at St. Helena, and 1'·9 in arc at the Cape) and upwards, from the normal place, were separated from the others and the values of the differences taken; there were therefore two series of figures to be dealt with, viz. the number of disturbances, and the aggregate amount of disturbance. These were separated into disturbances of the north end of the magnet towards the east and towards the west, and the effect of each considered separately.

The periodical character of disturbances at St. Helena and the Cape in a cycle of years is indicated insofar as the limited extent of the observations would permit; sufficient however to point to the year

1843 as that of least disturbance at these two places, by showing a regular decrease from the previous years, and an increase in every succeeding year of observation. Though the hourly observations were discontinued before 1848, the year which Colonel Sabine has shown to be that of periodical maximum, as 1843 was that of minimum magnetic activity at Toronto and Hobarton, the observations now discussed are shown to be quite consistent with this period, and thus tend to establish it as a general law cf magnetic phenomena. In the aggregate of each year the disturbances towards the west are shown to preponderate over those towards the east, both at St. Helena and the Cape of Good Hope; a similar preponderance of westerly over easterly has been found in every year of observation at Hobarton, but at Toronto the easterly disturbances exceeded the westerly both in number and amount in every year.

Arranging the disturbances into the several *months* of their occurrence, the greatest disturbance is found to occur in January and the least in June at St. Helena and the Cape of Good Hope; the same months being those of greatest and least disturbance at Hobarton, whereas at Toronto, *both* January and June are months of minimum disturbance, the maxima disturbance occurring there in April and September.

From this identity of the epoch of greatest and least disturbance, —at St. Helena, where the months of January and June are not those of opposite seasons, viewed either with respect to the sun's extreme altitude or to extreme periods of temperature,—at the Cape, situated in S. latitude 33° 56′,—and at Hobarton in S. latitude 42° 52′,—and contrasting this identity with a different law at Toronto in N. latitude 43° 39′, the author infers that the principal causes which produce an annual period of disturbance are not dependent upon local seasons. It is likewise pointed out that about the period of the equinoxes there is a tendency to maximum disturbances at all the stations, producing absolute maxima at Toronto, faintly but systematically indicated at the other stations.

The westerly disturbances were found to exceed the easterly in every month in the year at St. Helena and the Cape, which agrees with the results deduced from the Hobarton observations, while it appears from the observations at Toronto that the easterly disturbances exceeded the westerly in every month. The average value of a westerly disturbance is greater than that of an easterly in every month at St. Helena and the Cape of Good Hope. The disturbances at Hobarton again coincide with this result; and in a slight and less perfectly marked degree, Toronto has the same peculiarity.

Arranging the disturbances into the several *hours* of their occurrence, the hours of the day are found to be those of greatest disturbance in a very considerable degree; the sum of the ratios, during the twelve hours of the day, being about seven times as great as the sum of those in the twelve hours of the night at St. Helena, and about 2·6 times as great at the Cape of Good Hope; while at Hobarton the sum of the twelve night ratios slightly exceeded the day; at Toronto the excess was larger, viz. as 1·3 to 1. The laws of easterly and westerly disturbances, in relation to the local hours, are

then examined separately. At St. Helena and the Cape, the easterly day-disturbances exceed the easterly night-disturbances, and the westerly day-disturbances exceed the westerly night-disturbances. These results are compared with those at Toronto and Hobarton.

At St. Helena, although but comparatively few disturbances occur during the night hours, these disturbances are almost all westerly (183 disturbances, in all, occurred in nine night hours during five years, of which 174 were westerly and but nine easterly). In the day hours the westerly only *slightly* exceed the easterly disturbances. At the Cape, the westerly excess is less in the night and greater in the day than at St. Helena, and the night excess much greater than the day excess.

At St. Helena, the fact of the disturbances being more frequent in the day than in the night is consistent in every month of the year; this appears worthy of remark when it is remembered that at St. Helena the curve of the diurnal variation of the declination is precisely reversed at two opposite periods of the year; in one case corresponding to the curve of diurnal variation in middle northern latitudes, and in the other to that in middle southern latitudes.

The mean effect of the disturbances which have been separated as described, and which comprise all of largest magnitude, is a *constant westerly* effect at every hour both at St. Helena and the Cape of Good Hope, acting more energetically in the night than in the day. At Toronto the mean effect is westerly in the day and easterly in the night; at Hobarton, easterly in the day and westerly in the night.

March 3, 1853.

COLONEL SABINE, R.A., Treas. & V.P., in the Chair.

In accordance with the Statutes, the following List of Candidates for election into the Society was read by the Secretary:—

James Apjohn, Esq.
John George Appold, Esq.
Henry Foster Baxter, Esq.
Sir Edward Belcher.
John Allan Broun, Esq.
Alexander Bryson, Esq.
Antoine Jean François Claudet, Esq.
Edward J. Cooper, Esq.
Richard Cull, Esq.
Campbell De Morgan, Esq.
Solomon Moses Drach, Esq.
Robert Ellis, Esq.
E. Frankland, Esq.
John Hall Gladstone, Esq.
John Hawkshaw, Esq.
Robert Hunt, Esq.
Edward Augustus Inglefield, Esq.
Joseph Beete Jukes, Esq.
Edward Joseph Lowe, Esq.
Robert MacAndrew, Esq.
Charles Manby, Esq.
Robert William Mylne, Esq.
Henry Perigal, Jun., Esq.
Joseph Prestwich, Jun., Esq.
William John Macquorn Rankine, Esq.
Lovell Augustus Reeve, Esq.
William Wilson Saunders, Esq.
William Spottiswoode, Esq.
Captain Richard Strachey.
Count P. de Strzelecki.
Robert Dundas Thomson.
Charles Vincent Walker, Esq.

A paper was read, entitled "On the Meteorology of the English Lake District (Sixth paper, for 1852)." By John F. Miller, Esq., F.R.S. &c. Received February 22, 1853.

This paper contains records of the meteorology of the Lake district, similar to those of former years which have been communicated by the author. These are given in tables:—Table I. is a Synopsis of the fall of Rain in the Lake district of Cumberland and Westmoreland in the year 1852. Table II. Wet days (the number in each month and the whole year at each station). Table III. The quantity of Rain received by the mountain gauges in the year 1852 (for each month). Table IV. The quantity of Rain for the summer months (May to October). Table V. For the winter months (November to April). Table VI. Temperature (max. min. approximate mean) at Seathwaite, Borrowdale, 368 feet above the sea-level (for each month and for the year). Table VII. Temperature at Whitehaven. Table VIII. Minimum temperature of each month, on Sca Fell Pike and Gabel, and at Sprinkling Tarn, from July 1851 to December 1852 inclusive. Table IX. Monthly hygrometrical observations taken at the mountain stations adjacent to the Vale of Wastdale, in the year 1852. Table X. Deductions relative to the humidity of the atmosphere at the mountain stations in 1852. Tables XI., XII., XIII., XIV. contain hygrometrical observations made at various stations in April and July 1848, and in December 1850 and July 1851.

In the remarks which follow the tables, the author states that the past year is distinguished by several marked peculiarities, of which the most prominent are—the very large amount of rain and its very unequal distribution over the different seasons; the enormous and unprecedented fall in the first two and last two months; and the protracted drought of ten weeks in the spring, the longest, though not the most severe which has occurred in the northern counties within the memory of the existing generation. The year is further remarkable for its high temperature; the large amount of surface evaporation; the great heat of July and August; the great quantity of free electricity, as manifested by the unusual number and almost tropical severity of the thunder-storms; the small number of frosty nights, and the entire absence of snow; and, lastly, for the violent gales of wind which prevailed during the last week of December, particularly the hurricane on the morning of Christmas-day. After the discussion of these irregularities, tables are given showing the excess or deficiency per cent. of the principal mountain gauges over or under the quantity of rain received by the adjacent valleys, both in the summer and winter months, in each year since the instruments were erected in 1846. The remainder of the paper is occupied with details referring to the temperature and the hygrometrical observations at the mountain stations.

March 10, 1853.

LORD WROTTESLEY, V.P., in the Chair.

The following letter, addressed to Michael Faraday, Esq., and by him communicated to the Society, was read :—

Dunse (N. Britain), March 1, 1853.

Dear Sir,—In the report in the Athenæum of your lecture at the Royal Institution on the 21st of January, I observe that you refer to the highly interesting observations of Schwabe, Sabine, Wolf, Gautier, &c., from which it would appear that a connection exists between the solar spots and the variations of the terrestrial magnetic forces. Since a connection has been demonstrated to exist between the latter and auroral phenomena, I was induced to look over my notes relating to the auroræ observed at this place, with a view to ascertain whether these also exhibited maxima and minima, and if so, whether the periods of such agreed with those of the solar spots and of the magnetic variations. The subjoined table shows the distribution of the auroræ seen here in the years 1838 to 1847 inclusive :—

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Sum.
1838	5	3	4	3	...	...	...	2	4	1	2	3	27
1839	9	1	2	4	1	...	...	...	11	7	2	1	38
1840	5	5	2	4	...	...	...	3	7	6	6	5	43
1841	6	3	4	4	2	...	...	3	3	3	7	7	42
1842	2	2	...	...	...	...	1	...	3	...	1	...	9
1843	2	1	1	1	...	...	...	2	...	3	...	...	10
1844	1	...	2	...	...	...	...	...	1	3	4	2	13
1845	1	2	...	1	...	...	...	1	2	1	1	1	10
1846	...	1	...	...	...	...	1	2	7	4	1	...	16
1847	2	2	3	1	...	...	...	1	5	6	6	4	30
	33	20	18	18	3	...	2	14	43	34	30	23	238

These figures speak for themselves. I may remark that the returns for 1842 are incomplete, as I was absent from home during March and April of that year. In 1848 I was also absent for some months, but from the number of auroræ which I have noted during that year, I am satisfied that a maximum then occurred, both as regards the number and the intensity of auroral displays. This present winter has been very barren in auroral phenomena.

Of crimson auroræ I find I have noted two in 1837, one in 1839, one in 1846, three in 1847, and no less than six in 1848.

A discussion of the auroræ seen in North America and the North of Europe during a series of years would be interesting with reference to the points in question.

Apologizing for troubling you,

I am, dear Sir,

With the greatest respect, yours faithfully,

Wm. Stevenson.

The following communication was likewise read:—" On the Reproduction of the Toad and Frog without the intermediate stage of Tadpole." By Edward Joseph Lowe, Esq., F.G.S., F.R.A.S. Communicated by J. Lee, LL.D., F.R.S. &c. Received February 19, 1853.

The following brief remarks on the Toad (*Bufo vulgaris*) and the Frog (*Rana temporaria*) may perhaps be received with some degree of interest, as they are, I believe, contrary to the generally received notion of the procreation of these reptiles. Ray, and most naturalists, at least, consider toads and frogs as oviparous animals, yet it is apparent that they are viviparous as well, or if they do not bring forth their young alive, have the power of reproduction in a different manner to the ova and subsequent tadpole.

Mr. J. Higginbottom of Nottingham, who has paid great attention to this subject, has clearly proved the development of the tadpole to the perfect toad in situations wholly deprived of light, as I have through his kindness several times witnessed. My present remarks are intended to show that *occasionally* frogs and toads are reproduced in localities where it would be impossible for the intermediate stage of tadpole to have any existence.

First. *Toads deposit spawn in cellars and young toads are afterwards observed.*

Last summer several masses of spawn were procured from my cellar, having been found deposited amongst decaying potatoes, &c., and subsequently young toads were noticed. The cellar is free from water, and at a considerable distance from any brook.

Secondly. *Young toads are observed about hot-beds.*

In the kitchen-garden at Highfield House (which is entirely walled round) young toads have been noticed about the cucumber- and melon-beds. The gardeners have been in the habit of bringing toads to these beds to destroy the insects; these have continued amongst the warm damp straw all summer. It is after these beds have remained three or four months that the young ones have been noticed. Toads would have to travel nearly half a mile to reach this garden from the brook or lake, and also to mount a steep hill, besides taking the opportunity of coming through the door. Toads so small are not seen in any other part of the gardens.

Thirdly. *Young toads and frogs observed in abundance at the summit of another hill, whilst quite small.*

During the past summer, especially in the month of July, very many young toads and frogs were seen amongst the strawberry plants, apparently from a week to a month old. These might possibly have travelled from the brook a few hundred yards distant; yet it is strange, that with the exception of these beds, no young toads could be found elsewhere in the garden. A number of full-grown toads are mostly to be seen about these beds.

Fourthly. *Young frogs dug out of the ground in the month of January.*

In digging in the garden amongst the strawberry-beds (near where so many toads were observed last summer) in the middle of

January in the present year, a nest of about a score young frogs were upturned. These were apparently three or four weeks old. This ground had been previously dug in the month of August and many strawberry plants buried; it was amongst a mass of these plants in a state of partial decomposition that these young ones were observed.

Fifthly. *Young frogs are bred in cellars where there is no water for tadpoles.*

In mentioning this subject to Mr. Joseph Sidebotham of Manchester (an active botanist), he informed me that young frogs, and in fact frogs of all sizes, were to be seen in his cellar amongst decaying dahlia tubers. The smallest of them were only about half the ordinary size of the young frog when newly developed from the tadpole. He further stated that there was no water in the cellar, and no means of young frogs entering, except by first coming into the kitchen, a mode of entry, if not impossible, highly improbable. Mr. Sidebotham never found any spawn.

It seems probable from the above, that frogs are occasionally born alive in situations where no water can be found for the spawn to be deposited in, and that toads are either reproduced in the same manner, or from the egg directly. The latter mode seems most likely, owing to spawn having been found previously to the young toads.

Mr. Higginbottom tells me, the same remark on the birth of the Triton, without the stage of tadpole, has been mentioned to him.

These are the facts; should the subject be deemed worthy of further investigation, I shall be glad to continue observations upon these reptiles during the present year, or to make any experiments that may be deemed advisable.

March 17, 1853.

COLONEL SABINE, R.A., Treas. & V.P., in the Chair.

The Right Honourable Viscount Palmerston was balloted for and elected a Fellow of the Society.

The following papers were read:—

1. "On Animal and Vegetable Fibre as originally composed of Twin Spiral Filaments, in which every other structure has its Origin; a Note showing the confirmation by Agardh, in 1852, of observations recorded in the Philosophical Transactions for 1842." By Martin Barry, M.D., F.R.S., F.R.S.E. Received February 24, 1853.

After referring to the drawings to his paper on Fibre, published in the Philosophical Transactions for 1842, and the opinions entertained by physiologists regarding the peculiar views he advanced in that paper with reference to the original composition of organic fibre, the author states that, after the lapse of eleven years, these views have been fully confirmed, and in proof of this refers to a paper—"De cellula vegetabili fibrillis tenuissimis contexta" (Lundæ, 1852),

by Agardh. He further remarks, that his paper of 1842 contains a record of other observations made in a field beyond the region of Agardh's researches; observations which he thinks explain how it is that fibre forms the membrane of the cell, and, what he deems of more importance still, the mode of origin of fibre. He refers generally to the drawings in that paper, from which, in connection with facts previously recorded in the Philosophical Transactions, he states that it appears—1, that fibre has its origin in the so-called "cytoblast," the outer part of which always passes into a ring or coil of fibre; 2, that when a cell is to arise, its primary membrane is formed out of this ring or coil of fibre; 3, that then the nucleolus of the "cytoblast" becomes the nucleus of the cell; 4, that the outer part of the nucleus of the cell also passes into a ring or coil of fibre, wherewith to form deposits such as the annular and spiral, or to weave the secondary membranes; 5, that the term "cytoblast" is unsuitable, as the body so called does not always become a cell; 6, that fibre is thus more universal as well as more primitive even than the cell, for fibre not only forms the cell, but it forms other structures without having first to form a cell; 7, that the prime mover in both the "cytoblast" and the nucleus is the *nucleolus*, which is the organ of absorption, assimilation, and secretion; 8, that the nucleolus is continually giving off its substance and continually renewing it, continually passing from the state of nucleolus into that of "cytoblast" or nucleus,—so that the "cytoblast" and the nucleus are each of them but the nucleolus enlarged; 9, that it is therefore the nucleolus enlarged that passes into fibre; 10, that the nucleolus always passes into fibre, and directly into no other form than that of fibre; 11, that thus the whole organism arises out of nucleoli, for fibre is but the nucleolus in another shape, and every structure arises out of fibre; 12, that the nucleolus is reproduced by self-division, and that subsequently, when it has passed into the form of fibre, the mode in which the nucleolus gives origin to other structures is such as to imply even here the continued reproduction of its own substance—that mode being self-division.

The author describes particularly the mode of origin of primary and secondary membranes, and division of the cell. He considers that the latter is initiated by self-division of the nucleolus into halves which become "cytoblasts," and it is completed by the formation out of these of two young cells, the walls of which, where in contact with one another, form a septum dividing the parent cell into two compartments. Thus for division of the cell there occurs no folding inwards of a "primordial utricle," as maintained by Von Mohl, nor any division of the contents of a parent cell into two parts, around which contents are formed the walls of two young cells, as supposed by Nägeli and Hofmeister. On the subject of annular, spiral, and other deposits in the vessels of plants, the author remarks, that when the divisions of an annular or spiral fibre are not continued, but partial and irregular, we have the reticular form, as well as an explanation of the supposed tendency in vegetable fibre to anastomosis.

The two spiral filaments composing fibre at first appeared to the

author to run in opposite directions, which he subsequently saw was not the case,—their direction is the same. This error he corrected in Müller's Archiv for 1850.

The author remarks, that observers in their endeavours to reach the *ultimate* structure of the muscular fibril have actually gone too far, and reached a later generation,—mistaking for the fibril a row of quadrilateral particles, the mere elements thereof. These particles, he observes, are known to be alternately light and dark in alternate order; they give origin to the term spirals; and for this purpose the dark particles undergo what observers have entirely overlooked, division and subdivision, which changes he has figured in Müller's Archiv, 1850. The preparation in which he has again met with the subdivision into four is still, the author states, in his possession for demonstration to others.

2. "On the penetration of Spermatozoa into the interior of the Ovum; a Note showing this to have been recorded as an established fact in the Philosophical Transactions for 1843." By Martin Barry, M.D., F.R.S., F.R.S.E. Received February 24, 1853.

Referring to a statement by Dr. Nelson, in a paper "On the reproduction of the *Ascaris Mystax*," that the investigations in that paper "appear to be the first in which the fact of the penetration of spermatozoa into the ovum has been distinctly seen and clearly established in one of the most highly organized of the Entozoa," the author of the present communication remarks, that when Dr. Nelson made this statement he was evidently not aware of what had been published on the subject. In proof of this Dr. Barry refers to his own paper, entitled "Spermatozoa observed within the Mammiferous Ovum" (Phil. Trans. 1843, p. 33), in which he states that he had met with ova of the Rabbit containing a number of spermatozoa *in their interior*; and to the Edinburgh New Philosophical Journal for October 1843, which contains a drawing in which seven spermatozoa are represented in the interior of an ovum, besides the statement that in one instance he had counted more than twenty spermatozoa in a single ovum. In conclusion he remarks, that Dr. Nelson merely added a further confirmation in ova of an entozoon, to what his own researches on mammiferous ova had enabled him to record as an established fact nine years before.

The Society then adjourned to the 7th of April.

April 7, 1853.

COLONEL SABINE, R.A., Treas. & V.P., in the Chair.

A paper was read, entitled "Observations on the Anatomy of the Antennæ in a small species of Crustacean." By John D. M^{c}Donald, M.D., Assistant Surgeon to H.M.S.V. Torch. Communicated by

Sir William Burnett, F.R.S., Director-General of the Medical Department of the Navy. Received March 3, 1853.

The little crustacean which is the subject of this paper was taken in considerable numbers in the voyage from St. Vincent to Rio Janeiro. There are several anatomical peculiarities mentioned, but the most remarkable is the structure of the right antenna of the male. These organs are in the female perfectly symmetrical, and resemble that of the left side in the male; and although in the very young state of the latter sex the right antenna differs but little in external appearance from the left, yet the peculiar hypertrophied condition of the modified segments in the corresponding organ of the adult male is to be distinctly traced in a rudimentary state.

As the animal lives in the open ocean, none of the limbs are adapted for walking; but when placed in a vessel of sea-water, they rested upon their antennæ on reaching the bottom, and paddled themselves about by their fore-limbs and tail.

The author remarks that in all their movements the males exhibit a tendency to turn towards the left side, and concludes the rationale of this fact to be, that the brain on the right side being more developed at the part from which the right antenna derives its nerves, a corresponding predominance is given to the power of the locomotive organs on that side.

When fully developed, each antenna in both sexes consists of twenty-five segments. Of these, the first thirteen present nothing remarkable; but all the remaining pieces on the right side enter into the composition of the curious prehensile organ which forms the principal subject of the paper.

This organ is composed in the following manner:—The fourteenth and four following segments are dilated into a large flask-like organ, the neck of which is eked out by the nineteenth and twentieth. The next two segments are fused together, and are articulated with the foregoing by a simple joint, and the whole of the remaining segments form another piece similarly articulated with the intermediate piece; so that the whole results in two simple joints susceptible of flexion in one direction only. On the eighteenth segment is a barbed process having its apex directed backwards, and its anterior border beset with sharp teeth. Two processes of the same nature, but differently placed and more elongated, lie side by side upon the fore-part of the first compound segment. This piece and that which succeeds it act upon each other like a pair of jaws, each furnished with an array of sharp conical teeth, while the last compound member of the series plays over the upper surface of the eighteenth segment.

The author then proceeds to describe the muscles which move this complex apparatus. The extensors are small and feeble, but the flexors are, as might be anticipated, more complex and powerful. They are two in number. The first has its origin in the large flask-like dilatation, and is inserted by a tendon into the second compound piece, from which the second muscle arises, and is inserted, also by tendon, into the third piece. The paper is illustrated by elaborate drawings.

April 14, 1853.

COLONEL SABINE, R.A., Treas. & V.P., in the Chair.

The Right Honourable Viscount Palmerston was admitted into the Society.

A paper was read, entitled "On certain Functions of the Spinal Chord." By J. Lockhart Clark, Esq. Communicated by E. Solly, Esq., F.R.S. Received March 15.

These investigations were undertaken by the author partly with the view of settling the long-agitated question whether all the roots of the spinal nerves terminate in the spinal chord, or whether any part of them ascend within the white or grey columns to the brain. The preparations employed for this purpose were made according to the new method described in the author's former communication, Phil. Trans. 1851, Part 2; and the animals selected were the Ox, Calf, Cat, Rat, Mouse and Frog. Of the spinal chord of the Cat, he has succeeded, after much trouble, in rendering transparent longitudinal sections $\frac{1}{12}$th of an inch in thickness, and more than two inches in length, including the roots of four or five pairs of nerves.

The principal results at which the author has arrived are as follows:—

That the posterior roots of the spinal nerves consist of three kinds; two of these enter the posterior grey substance at right angles, and the third kind with different degrees of obliquity upwards, a small proportion of the latter taking a longitudinal course and becoming lost in the posterior white columns.

That in no instance were any of the fibres of the anterior roots seen to ascend with the anterior white columns, before they entered the grey substance.

That besides the transverse bundles that form the anterior roots, a continuous system of exceedingly fine transverse fibres issue from the anterior grey substance and become lost as they proceed towards the surface of the chord.

That from the preceding facts, it may be inferred that nearly all, if not the whole of the fibres composing the roots of the spinal nerves proceed at once to the grey substance of the chord; and that if any of them ascend *directly* to the brain, it must be *those only* of the *posterior* roots which run longitudinally in the posterior white columns.

That the communication between the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral.

That many of the fibres which belong respectively to the anterior and posterior roots in different regions of the chord, terminate there by forming with each other a series of loops of various sizes and lengths; and that it is not improbable that some of them may reach even as far as the brain, as it is well known that the formation of loops is one mode in which nerve-fibres do terminate there. A por-

tion of the roots however may be connected with the vesicles of the chord, although the evidence of any such connection is very unsatisfactory.

That there are reasons for believing that the grey substance of the chord does not transmit impressions to and from the brain; and that the fine longitudinal fibres described by Stilling have not been found by the author.

That there is a great correspondence in the fibrous arrangement between the grey substance of the chord and the chiasma of the optic nerves. That the fact that the nerve-roots not only diverge both upwards and downwards to a considerable distance beyond their point of entrance, but intermingle also with each other in the most intricate manner, may explain how impressions made at one particular spot are communicated in different directions to distant parts of the chord, so as to excite a simultaneous and sympathetic action in classes of muscles which otherwise would appear unconnected.

April 21, 1853.

The EARL OF ROSSE, President, in the Chair.

The following letters were read:—

1. Extract from a letter from M. Regnault to Col. Sabine, R.A., Treas. R.S.

Mon cher Col. Sabine,—Je reçois aujourd'hui les Transactions Philosophiques de 1852, et j'y vois, avec surprise et avec une grande satisfaction, que la Société Royale de Londres m'a fait l'honneur, au mois de Novembre dernier, de me nommer un de ses membres étrangers. Jusqu'ici je n'en avais reçu aucun avis; je viens donc vous prier de vouloir bien remercier, en mon nom, la Société Royale pour le témoignage d'estime qu'elle m'a donné, et de lui indiquer en même temps les circonstances qui m'ont empêché de lui adresser plustôt ces remerciments.

J'ai trouvé dans les Transactions de 1852 un mémoire de MM. Joule et Thomson sur la chaleur spécifique de l'air atmosphérique, et des expériences des mêmes auteurs sur les effets calorifiques qui se produisent pendant le passage de l'air à travers de petites ouvertures. Je me suis occupé depuis longtemps de ces divers sujets, et, depuis plusieurs années, tout ce qui se rapporte à ce point est terminé. Si je n'ai pas publié mes résultats jusqu'ici, c'est qu'ils forment une très petite partie de mes recherches qui m'occupent depuis six ans, et que mon intention est de publier dans leur ensemble. Elles formeront un volume des mémoires de notre Académie; leur publication est longue à cause des calculs numériques énormes qu'il a fallu exécuter. Cette publication s'est d'ailleurs trouvée fortement entravée par mes nouvelles fonctions de Directeur de la manufacture de Sèvres que j'ai acceptée bien malgré moi, prévoyant bien les obstacles qu'elles présenteraient à l'exécution de mes travaux scientifiques. Mais, enfin,

aujourd'hui, l'impression de cette suite de mémoires est commencée, et je ferai tous mes efforts pour les mettre au jour le plustôt possible. Je vous donne ici la liste des sujets que j'y ai traités.

1. Nouvelles recherches sur la compressibilité des gaz. Elles se rapportent à un certain nombre de gaz qui n'avaient pas été étudiés dans mon premier mémoire sur ce sujet, et dont j'avais besoin de connaitre les lois de compressibilité pour mes recherches subséquentes.

2. Sur les forces élastiques des vapeurs depuis les températures les plus basses jusqu'à celles qui correspondent à une tension de 12 atmosphères. Ces expériences se rapportent à un grand nombre de liquides autres que l'eau, et qui sont à peu près tous ceux que nos moyens chimiques nous permettent aujourd'hui d'obtenir à l'état de pureté.

3. Sur les forces élastiques des vapeurs produites par les mêmes liquides mélangés. Ces expériences m'ont conduit à des résultats curieux, souvent en contradiction complète avec les idées actuellement admises dans la science.

4. Sur les forces élastiques des mêmes vapeurs dans les gaz à saturation et à non saturation.

5. Sur les chaleurs latentes des mêmes vapeurs sous diverses pressions depuis les pressions les plus faibles jusqu'à la pression de plusieurs atmosphères.

6. Sur la chaleur latente de vaporisation de ces mêmes liquides, quand ils se vaporisent dans les gaz à saturation ou à non saturation.

7. Sur les chaleurs spécifiques de ces liquides depuis les températures les plus basses jusqu'à celles de leur ébullition.

8. Sur les chaleurs spécifiques des gaz, la pression de l'atmosphère, et entre les diverses limites de température. Ce travail m'occupe depuis plus de dix ans et m'a présenté un grand nombre de difficultés. Il est toujours facile d'obtenir des évaluations approchées de ces éléments ; la difficulté ne commence que quand il s'agit de décider si le nombre que l'on obtient n'est pas trop faible, ni trop fort. Un expérimentateur habile trouve facilement des nombres identiques en opérant toujours de la même manière ; mais l'identité des résultats ne prouve nullement leur exactitude. Les erreurs accidentelles sont toujours faciles à éviter ; il n'en est pas de même des erreurs constantes et de celles qui tiennent au procédé employé. Le nombre trouvé par Mr. Joule pour la chaleur spécifique de l'air sous pression constante (0·226) est beaucoup trop faible. Celui qui résulte de mes expériences très nombreuses, et faites dans des circonstances variées, afin de reconnaitre et d'éliminer les erreurs constantes, est 0·237. Je l'ai publié à l'Académie des Sciences depuis plusieurs mois lorsque je lui ai présenté mon second mémoire sur l'hygrométrie où cet élément était nécessaire. Mon travail comprend la chaleur spécifique d'un grand nombre d'autres gaz ; tels que l'oxygène, l'hydrogène, le chlore, le brôme, l'acide carbonique, l'oxyde de carbone, le protoxyde d'azote, le deutoxyde d'azote, les hydrogènes carbonés, l'acide sulfureux, l'acide sulfhydrique, l'acide chlorhydrique, l'ammoniaque, &c. &c.

9. Sur les chaleurs spécifiques des gaz sous des pressions plus grandes que la pression de l'atmosphère. Ce travail a pour but de décider si la chaleur spécifique d'une même masse de gaz reste constante sous les différentes densités que l'on peut donner à ce gaz, et de trouver la loi de la variation.

10. Sur les chaleurs dégagées ou absorbées par l'air pendant sa compression ou son expansion. Ce travail est celui qui m'a le plus longtemps arrêté et qui m'a empêché de publier l'ensemble de mes recherches. C'est à cette occasion que j'ai étudié les phénomènes calorifiques qui se produisent pendant le passage des gaz comprimés à travers de petites ouvertures ou par des tubes capillaires, suivant que le gaz est chaud ou qu'il est froid. J'ai étudié le même sujet sur le gaz qui produit un travail moteur—

1. Complètement intérieur au calorimètre.

2. Extérieur au calorimètre et produisant un effet mécanique qui puisse être assigné.

Je suis encore arrêté dans les conclusions à déduire de ce dernier travail, par suite de plusieurs anomalies, dont je ne trouve l'explication par aucune des théories proposées.

11. Sur les chaleurs spécifiques des vapeurs sous diverses pressions. J'ai soumis à ces recherches un très grand nombre de vapeurs, mais en insistant plus spécialement sur celles qui peuvent être obtenues à l'état de pureté parfaite, et hélas! elles ne sont pas très nombreuses. La chaleur spécifique que j'ai trouvé à la vapeur d'eau sous la pression de l'atmosphère est 0·475 par rapport à l'eau liquide.

L'ensemble de ces recherches permettra de calculer numériquement les effets mécaniques que l'on peut retirer théoriquement de 1 kil. de charbon, suivant que l'on applique la chaleur à la production de la vapeur d'eau ou d'une autre vapeur, sous diverses pressions, à saturation ou sous détente, ou qu'on l'applique à un gaz quelconque à détente ou sous pression constante. En un mot, j'espère que les résultats obtenus dans ces recherches permettront de résoudre enfin, avec quelque certitude, les questions qui divisent depuis si longtemps les ingénieurs, en même temps qu'elle fourniront un grand nombre d'épreuves pour décider les diverses théories qui ont été proposées pour la chaleur.

Je vous prie, mon cher Colonel, de donner lecture de cette lettre à la Société Royale, et d'en demander l'insertion dans ses publications, si elle n'y trouve pas d'empèchement.

J'ai l'honneur d'être,

Votre dévoué serviteur,

V. Regnault.

2. Extract from a letter from M. Kämtz addressed to Colonel Sabine (Translation):—Received April 10, 1853.

Dorpat, 15th March (N. Style), 1853.

I have just completed the memoir on terrestrial magnetism which I mentioned to you in former letters. It would follow from the materials which I have employed, that the horizontal force requires constants which differ from those for the vertical force, and the

differences, although for the most part not considerable, are sometimes too large, as it appears to me, to be overlooked: it is for observers to decide in this matter. To avoid the introduction of insecure numbers I have not computed the potential, but have contented myself with deriving the values of X, Y and Z (declination, inclination, and force horizontal and total) being the quantities principally used for comparison. A second calculation of the constants for X and Y would have given rather more exact values, but I think that with the existing data the result would scarcely repay the labour of so extensive a calculation, for the tables would have to be entirely recomputed.

After obtaining the data from the theory, I wished to compare them with observation. I could take for the inclination and force the values which had served as the bases of the calculation; there are indeed several observations of inclination without our knowing the force, and a few of the force without inclination; but on the other hand, there are more extensive districts where I had no declinations, although the other two elements were well determined. Under these circumstances I was obliged to have recourse to older observations, which indeed I had occasion to use also for secular change and reduction for epoch to 1830. Thus the character of the work was gradually modified. For the Atlantic Ocean I availed myself of your excellent Memoir, Contributions No. IX.; I only added the observations of Lütke, d'Urville, and Rumker; of older observations, I took those of Abercrombie and Ekeberg, only using them however in parts where they had observed the inclination, and principally with the view of finding its secular change.

In the Pacific there was more deficiency; with the exception of what has been furnished by Lütke, d'Urville, the ships of the Prussian Merchant Service, and in the neighbourhood of the magnetic equator by Duperrey, modern voyages scarcely gave me anything; for Becquerel, in his detailed table*, omits all determinations made at sea I therefore took all I could get since the voyages of Byron, Carteret and Wallis, chiefly from Hansteen's great work, and reduced to 1830, taking from each observer the mean of all his determinations within a space of 5° lat. and 10° long. I did not allow myself to exclude any observer, for as all have errors from the ship's iron, it might easily happen that a man but little known may have given values which are much nearer to the truth than those given by a celebrated voyager. A selection is no less objectionable, as it might easily lead to retaining observations which accord with a possibly not altogether correct theory, and omitting others, otherwise good, which might depart from it. Moreover, such a full comparison shows better what may be expected from the observations: incomplete as are the data they afford, yet, when I arrange the remaining errors in latitude and longitude, I rarely find groups in which the errors show a certain degree of systematic character over extensive districts, so as to alter the declination or inclination a degree.

* Electricité, tome vii.

The inclination appears to me to be very incorrect in the meridian of Alexandria. In India the few results of Elliot's given in the Phil. Mag. from the Proceedings of the Royal Society, accord extremely well with my calculations. I subjoin a few points of comparison:—

North Pole in 70° 7′ N., 263° 37′ E. Ross's observations give 70° 5′ N., 263° 14′ E. For the South Pole, 74° 6′ S., 152° 47′ E. seems to agree less closely with Ross's map, although the inclinations observed in the neighbourhood agree well with the calculated ones.

Maximum of total force in the northern hemisphere	1871·8 in	54° 21′ N.	265° 53 E.
(Error compared with Lefroy's observations	+6·4 in	54 1 N.	260 1 E.
	+0·8 in	55 6 N.	267 4 E.;
	+4·6 in	50 2 N.	264 0 E.)

For the force in the southern hemisphere, 2027·6 in 64° 36′ S., 144° 34′ E. is perhaps less correct, but there are in the S. hemisphere some striking anomalies in the observations.

The probable error of the force is 19·1; if however some groups which are at any rate not very correct are omitted, it becomes a little less than 15. For the inclination, 31′·1; for the declination, 41′·3.

I have gained my principal object in the work by becoming myself better informed, but I have thought it might perhaps not be wholly useless to others if only to stimulate travellers to make observations for comparison, and I have therefore been at the pains of writing out the table of comparisons (a task more irksome than that of calculation). But to publish is very difficult; on such a subject, and under present political circumstances, no bookseller in Germany would undertake it. The Admiralty at St. Petersburgh would perhaps print it in their Memoirs, but in the Russian language, in which it would be as good as buried; I turn to you, asking whether it might not be possible to get it published in England, not as a separate work, for perhaps no bookseller would accept it, but in a collection of similar papers. To me it would be most agreeable if it could appear either in the Reports of the British Association or in the Philosophical Transactions; and by preference in the latter, because I have derived from thence the best part of my materials, besides which the first results have already appeared in the Proceedings of the Royal Society. You will be best able to judge for me; I do not know what is the practice of either of those bodies in regard to writings not by their own members. My memoir is, I fear, long, but yet it consists chiefly of the table of comparisons. Of maps, I give declination, inclination and total force, two of each (Mercator and Polar). Unluckily I have no translator here who understands the subject, so I could only make the attempt myself, and as the memoir consists chiefly of figures from whence simple deductions are derived, there cannot be great errors; in the rest pray make the needful alterations where there are glaring Germanisms, if you think it possible the whole may be printed. If not, pray send me back the MSS. at some future time, not for publication, but for my own use. I finished yesterday, and send this week to the British Embassy; only I repeat my prayer,

that you will receive the work kindly, and tell me of its fate as soon as you can.

How far advanced is the Magnetic Survey of India? Could not you get travellers to observe in Egypt, Nubia, Mesopotamia and Persia? In the Pacific we want observations, especially on Erman's route: the number of facts known is too small for good inferences: we want tests for Duperrey's and Freycinet's Dips, which at neighbouring points differ 3° or 4°. Of Elliot's I know only the results taken from the Proceedings of the Royal Society; Kupffer has, I see, a copy of the work itself from the Phil. Trans. Has not Rae made observations on his journeys, and is anything printed? the same question as to all the expeditions in search of Franklin?

(Signed)

L. M. Kämtz.

April 28, 1853.

The EARL OF ROSSE, President, in the Chair.

A paper was read, entitled "On the Application of the Law of the Conservation of Energy to the Determination of the Magnetic Meridian on board Ship, when out of reach or out of sight of Land." By W. J. Macquorn Rankine. Communicated by Colonel Sabine, R.A., Treas., V.P.R.S. &c. Received April 5, 1853.

The author states that, assuming that when a ship is swung completely round, so that her head bears exactly as it did at first, the magnetism of the ship, and that of the compass-needle return to their original condition, the following theorem is necessarily true:—

The mechanical power developed by the mutual action of the ship and of the compass-needle during a complete revolution of the ship, is equal to zero.

If ζ' be the apparent magnetic azimuth of the ship's head, east of north; α' the corresponding apparent magnetic azimuth of a distant fixed terrestrial object (or where no such object is visible, of a star, corrected by calculation for its apparent diurnal motion); α the true magnetic azimuth of the same object, so that $\alpha-\alpha'$ is the westerly deviation of the compass-needle; then the above theorem is expressed symbolically thus:—

$$0=\int_0^{2\pi}\sin(\alpha-\alpha')\,.\,d\zeta'=\sin\alpha\int_0^{2\pi}\cos\alpha'\,.\,d\zeta'-\cos\alpha\int_0^{2\pi}\sin\alpha'\,.\,d\zeta';$$

from which it follows that

$$\tan\alpha=\frac{\int_0^{2\pi}\sin\alpha'\,.\,d\zeta'}{\int_0^{2\pi}\cos\alpha'\,.\,d\zeta'}.$$

The author remarks, that for the integrals in this formula are to be substituted, in practice, the algebraical sums of the sines and

cosines respectively, of the apparent magnetic bearings of the distant object, observed with the ship's head successively on the sixteen principal points of the compass (or on eight principal points, as the case may be). He considers that this method may prove useful in magnetic surveys of the ocean.

Additional remarks to the foregoing paper. Received April 14, 1853.

In consequence of a suggestion of Professor William Thomson, the author here investigates the modifications required in the formulæ of the previous part of his paper, when the compass-needle produces by induction a sensible effect on the mutual magnetic action of the earth and the ship.

Let A, as in Mr. Archibald Smith's formulæ, represent the mean of the sines of the deviations of the compass-needle observed during a complete revolution of the ship. As there is reason to believe that this quantity does not vary for a given ship in different parts of the earth so long as the quantity and distribution of her iron are unchanged, it may be determined, once for all, while in port, in the usual way.

When the ship is out of reach of land, let s be the mean of the sines, and c the mean of the cosines, of the apparent magnetic azimuths of a distant object observed during a complete revolution of the ship. Then the sine of the true magnetic azimuth of the object is given by the formula

$$\sin\alpha=\frac{s\sqrt{(c^2+s^2-\mathrm{A}^2)}-c\mathrm{A}}{c^2+s^2}.$$

When $\mathrm{A}=0$, this formula becomes

$$\tan\alpha=\frac{s}{c},$$

being identical with that of the previous part of this paper.

May 12, 1853.

The EARL OF ROSSE, President, in the Chair.

In compliance with the Statutes, it was announced from the Chair, that the following Candidates are recommended by the Council for election into the Society:—

James Apjohn, M.D.
John George Appold, Esq.
John Allan Broun, Esq.
Antoine Jean François Claudet, Esq.
Edward J. Cooper, Esq.
E. Frankland, Esq.
John Hall Gladstone, Esq.
Joseph Beete Jukes, Esq.
Robert MacAndrew, Esq.
Charles Manby, Esq.
Joseph Prestwich, Esq.
William John Macquorn Rankine, Esq.
William Wilson Saunders, Esq.
William Spottiswoode, Esq.
Count P. de Strzelecki.

A paper was read, entitled "A few Remarks on Currents in the Arctic Seas." By P. C. Sutherland, M.D. Communicated by Colonel Sykes, F.R.S. &c. Received April 16, 1853.

The author states that, during a voyage lately made in the Arctic seas, his attention was arrested by the power exerted by refrigeration and congelation in separating from water any saline ingredients it may contain, and of thus causing disturbances in the mean density of the waters of the ocean, which, after being influenced by currents, can be overcome only by subsequent intermixture with water from other localities where the disturbance in the equilibrium is of an opposite character. He considers that evaporation, which is so active within the tropical and temperate zones, obviously renders the sea more dense by depressing its surface, and thus gives rise to the necessity for currents from the two poles of the earth, where deposition of vapour predominates to a considerable extent over evaporation. This he illustrates by referring to the constant current from the Atlantic into the Mediterranean, caused by the evaporation in this sea preponderating over the supply of fresh-water. He then points out the necessity also of a current out of this sea, in order that its waters, by the constant influx of saline matters, may not become a saturated solution of the salts of the ocean; and infers that counter-currents into the polar seas must also exist to obviate the contrary tendency which the waters of these seas have to become fresh. He calls attention to the importance of ascertaining the differences that occur in many parts of the surface of the ocean in respect to its saline contents, that we may be enabled to determine to what extent the currents and counter-currents may be influenced by the comparative freshness of the iced water of the northern and southern regions, and the necessary saltness of the equatorial and other overheated basins. On this point, with respect to the Arctic seas, he refers to observations by Dr. Scoresby, Sir Edward Parry, and those recorded in tables appended to this paper, which have been extracted from the Meteorological Journal kept in the North Atlantic and Davis's Straits during the late voyage in the Isabel.

The author next refers to the remarkable difference occurring in the climate of the east and west sides of Davis's Straits, that of the latter being much the colder. In the absence of thermometric registers for the west, to compare with those on the east side, he points out how the appearance of the land and development of plants and land animals on the two coasts enable us to determine which has the warmer climate. Looking from the top of Baffin's Bay, which commands a good view of both shores, the east side at the sea-coast has many portions of land free from snow, whereas the opposite, by its snowy and icy covering, presents an appearance altogether uncongenial. On the former are found a tolerably abundant flora, hares and deer; on the latter, there scarce appears to be a spot to receive the roots of plants or the feet of these animals; and in the productions of the sea, both vegetable and animal, the same disproportion is met with. Upon the whole, he considers complete the analogy that exists between the North Atlantic and Davis's Straits,

both with respect to the climate of their shores and to their inhabitants of the animal and vegetable kingdoms. With reference to the question how this analogy is brought about, the author considers it difficult to decide whether the increase in the temperature of the water and the consequent improvement of the climate, on the east side of the strait, arise from the disposition the ice has to leave the coast, by which means the water becomes exposed to the influence of the sun; or from currents of heated water from a more southern region. He further remarks that its density here cannot be restored, if once disturbed, without admixture with a large volume of water somewhat above the mean density.

Again referring to the observations of Sir Edward Parry and those recorded in the tables, the author remarks that from these it will be seen that refrigeration has the effect of precipitating the salts of sea-water; and further, that it appears to him very probable that the temperature at which water begins to expand by the continued application of cold is that at which saline and earthy matter begins to be precipitated in solutions of the density of sea-water.

From the immense depth to which icebergs extend in Davis's Straits, and also from their vast number, the author infers that the temperature of the water will be kept pretty uniformly the same throughout a considerable part of its depth, rarely exceeding $+32°$, except at the surface, where the action of the sun comes into operation, in which case the water of greatest density from saline contents would always occupy the lowest position. In illustration of his views, he describes experiments on the freezing of sea-water of the density 1·025, in glass tubes; and from these he infers that, not only does congelation precipitate the saline matter in water, but refrigeration also at temperatures from 40° down to 32°. With reference to the influence of the density of the sea-water on currents, he remarks that after the warm season has fairly set in, in the Arctic seas, nothing is more common than to observe the surface-water, in hollowed out lanes or fissures of the land-ice, moving slowly towards the open water at the edge of the fixed ice; and this seaward motion is altogether independent of tidal motion or oceanic current, depending entirely upon the diminished density of the surface-water.

In conclusion, the author states that he does not know that we are yet in a position to demonstrate the actual existence of currents *into* the icy seas, as well as *out* of them; but that the necessity for them is obvious. It is not necessary, he remarks, that these currents, as in other parts, should occupy the surface, and probably also the bottom of one of the sides of the basins whose waters require to be renewed, as the Gulf-stream occupies the east side of the North Atlantic. It is plain that the cold and hot waters of two regions can be exchanged by the latter passing underneath the former; and although the arctic current from the Greenland sea does not contain much ice to the southward of Cape Farewell, it is more than probable its chilly waters pass over a fork of the Gulf-stream, which ultimately sweeps along the shores of West Greenland.

The Society then adjourned to the 26th of May.

May 26, 1853.

The EARL OF ROSSE, President, in the Chair.

The following communications were read :—

1. A letter from Mr. Joule to Colonel Sabine. Communicated by Col. Sabine, Treas. V.P.R.S. &c.

Acton Square, Salford, May 23, 1853.

My dear Sir,—I notice in the Proceedings of the Royal Society for April 21, a letter from M. Regnault in which some experiments of my own are referred to in a manner which I feel does me injustice. M. Regnault says, "Le nombre trouvé par M. Joule pour la chaleur spécifique de l'air sous pression constante (0·226) est beaucoup trop faible. Celui qui résulte de mes expériences très nombreuses, et faites dans des circonstances variées, afin de reconnaître et d'éliminer les erreurs constantes, est 0·237."

Now, in my paper on the air-engine, Phil. Trans. 1852, part i. p. 74, I have given the results of three series of experiments, viz. 0·23008, 0·22674, and 0·2325, and remark, "The mean of the three results is 0·22977, or nearly 0·23, which we may take as the specific heat of air at constant pressure determined by the above experiments."

I had been informed that M. Regnault was working on the specific heat of gases, and on that account did not feel it desirable to enter upon the laborious investigation which would have been requisite in order to add a couple of decimal figures to the number I had arrived at, and which was sufficient for the object I had in view, viz. to show that the discrepancy between the actual and theoretical velocity of sound arose from the incorrectness of Delaroche and Berard's determination of the specific heat of air (2·67), and not from any notable error in my number for the mechanical equivalent of the thermal unit. Having succeeded in doing this, I calculated the Tables 3 and 4 of my paper, using 0·238944 for the specific heat of air under constant pressure. I feel much gratified that the result arrived at by so eminent an experimentalist as M. Regnault confirms the accuracy in the main of the number I adopted.

I have only to add that Professor Thomson and myself, in pursuing our research on the thermal effects of rushing elastic fluids, are following up the views on the relation between mechanical and thermal phenomena originated by ourselves; and we shall feel most happy if M. Regnault's results, in the important line of investigation he has adopted, will facilitate our labour.

I have the honour to remain, dear Sir,

Yours most truly,

Colonel Sabine, &c. &c. &c. J. P. Joule.

2. "Experimental Researches on Vegetation." By M. Georges Ville. Communicated by The Earl of Rosse, P.R.S. Received May 26.

After stating that it has often been asked if air, and especially

azote, contributes to the nutrition of plants; and, as regards the latter, that this question has always been answered negatively, the author remarks it is however known that plants do not draw all their azote from the soil, the crops produced every year in manured land giving a greater proportion of azote than is contained in the soil itself. The question which he has proposed to himself for solution is, whence then comes the excess of azote which the crops contain, and in a more general manner, the azote of plants, which the soil has not furnished? He divides his inquiry into the three following parts:—

First. Inquiry into and determination of the proportion of the ammonia contained in the air of the atmosphere.

Second. Is the azote of the air absorbed by plants?

Third. Influence on vegetation of ammonia added to the air.

1. The author remarks that since the observation of M. Théodore de Saussure, that the air is mixed with ammoniacal vapours, three attempts have been made to determine the proportion of ammonia in the air: a million of kilogrammes of the air, according to M. Gräyer, contain 0·333 kil. $A2H^3$; according to Mr. Kemp 3·880 kil.; according to M. Frésenius, of the air of the day, 0·098 kil., and of night air, 0·169 kil. He states that he has shown the cause of these discrepancies, and proved that the quantity of ammonia contained in the air is 22·417 grms. for a million of kilogrammes of the air; and that the quantity oscillates between 17·14 grms. and 29·43 grms.

2. The author states that though the azote of the air is absorbed by plants, the ammonia of the air contributes nothing to this absorption. Not that ammonia is not an auxiliary of vegetation, but the air contains scarcely 0·0000000224, and in this proportion its effects are inappreciable. These conclusions are founded upon a great number of experiments in which the plants lived at the expense of the air without deriving any thing from the soil. For the present he confines himself to laying down these two conclusions:—1. The azote of the air is absorbed by plants, by the cereals, as by all others. 2. The ammonia of the atmosphere performs no appreciable part in the life of plants, when vegetation takes place in a limited atmosphere. After describing the apparatus by means of which he carried on his experiments on the vegetation of plants placed in a soil deprived of organic matter, and the manner in which the experiments were conducted, he adduces the results of these experiments in proof of the above conclusions.

3. With reference to the influence of ammonia on vegetation, the author states that, if ammonia be added to the air, vegetation becomes remarkably active. In the proportion of 4 ten-thousandths the influence of this gas shows itself at the end of eight or ten days, and from this time it manifests itself with a continually increasing intensity. The leaves, which at first were of a pale-green, assume a deeper and deeper tint, and for a time become almost black; their petals are long and upright, and their surface wide and shining. In

short, when vegetation has arrived at its proper period the crop is found far beyond that of the same plants grown in pure air; and, weight for weight, they contain twice as much azote. Besides these general effects there are others which are more variable, which depend upon particular conditions, but which are equally worthy of interest. In fact, by means of ammonia we can not only stimulate vegetation, but, further, we can modify its course, delay the action of certain functions, or enlarge the development and the modification of certain organs. The author further remarks, that if its use be ill-directed, it may cause accidents. Those which have occurred in the course of his experiments appear to him to throw an unexpected light upon the mechanism of the nutrition of plants. They have at least taught him at the expense of what care ammonia may become an auxiliary of vegetation. These experiments, which were made under the same conditions as those upon the absorption of azote, are then described, and their numerical results given.

To the conclusions already stated, the author adds that there are periods to be selected for the employment of ammonia, during which this gas produces different effects. If we commence its use when several months intervene before the flowering season of the plants, it produces no disturbance; they follow the ordinary course of their vegetation. If its use be commenced at the time of flowering, this function is stopped or delayed. The plant covers itself with leaves, and if the flowering takes place all the flowers are barren.

3. "An Account of Meteorological Observations in four Balloon Ascents made under the direction of the Kew Observatory Committee of the British Association." By John Welsh, Esq. Communicated by Colonel Sabine, R.A., Treas., V.P.R.S., President of the British Association, on the part of the Council of the Association. Received April 27th, 1853.

The object contemplated by the Kew Committee in the balloon ascents, of which an account is given in this communication, was chiefly the investigation of the variations of temperature and humidity due to elevation above the earth's surface. Specimens of the air at different heights were also obtained for analysis.

The instruments employed were the barometer, dry- and wet-bulb hygrometer, and Regnault's condensing hygrometer.

The barometer was a siphon, on Gay-Lussac's construction, without verniers; the upper branch of the siphon being alone observed, corrections having been previously determined for inequality of the tube at different heights of the mercury.

Two pairs of dry and wet thermometers were used, one pair having their bulbs protected from radiation by double conical shades open at top and bottom for the circulation of the air, the surfaces being of polished silver. The second pair were so arranged, that by means of an "aspirator," a current of air was made to pass over the bulbs more rapid than they would be exposed to by the mere vertical motion of the balloon. The object of this arrangement was to

enable the thermometers to assume with more rapidity the temperature of the surrounding air, and also to diminish the effect of radiation, in case the shades should not be a sufficient protection, especially when the balloon was stationary or rising very slowly. The thermometers used were very delicate, the bulbs being cylinders about half an inch long and not more than $\frac{1}{12}$th of an inch diameter. It was found on trial that when the bulbs were heated 20° above the temperature of the air in a room, they resumed their original reading in 40 or 45 seconds, when moved through the air at the rate of 5 or 6 feet in a second. It is thus probable that any error arising from want of sensibility in the thermometers will be small, and in all likelihood not more than may be expected from other accidental causes.

The observations were taken at short intervals during the *ascent*, it having been seldom practicable to obtain a regular series in the *descent*. The intervals were generally one minute, but frequently only 30 seconds, so that an observation was for the most part recorded every 200 or 300 feet. All the observations are given in detail in the tables accompanying the paper. They are also given in the graphical form in the curves.

The ascents took place on August 17, August 26, October 21, and November 10, 1852, from the Vauxhall Gardens, with Mr. C. Green's large balloon.

The principal results of the observations may be briefly stated as follows:—

Each of the four series of observations shows, that the progress of the temperature is *not* regular at all heights, but that at a certain height (varying on different days) the regular diminution becomes arrested, and for the space of about 2000 feet the temperature remains constant or even increases by a small amount: it afterwards resumes its downward course, continuing for the most part to diminish regularly throughout the remainder of the height observed. There is thus, in the curves representing the progression of temperature with height, an appearance of *dislocation*, always in the same direction, but varying in amount from 7° to 12°.

In the first two series, viz. Aug. 17 and 26, this peculiar interruption of the progress of temperature is strikingly coincident with a *large* and *rapid fall* in the temperature of the *dew-point*. The same is exhibited in a less marked manner on Nov. 10. On Oct. 21 a dense cloud existed at a height of about 3000 feet; the temperature decreased uniformly from the earth up to the *lower* surface of the cloud, when a slight rise commenced, the rise continuing through the cloud and to about 600 feet above its upper surface, when the regular descending progression was resumed. At a short distance above the cloud the dew-point fell considerably, but the rate of diminution of temperature does not appear to have been affected in this instance in the same manner as in the other series; the phenomenon so strikingly shown in the other three cases being perhaps modified by the existence of moisture in a *condensed* or vesicular form.

It would appear on the whole that about the principal plane of

condensation heat is developed in the atmosphere, which has the effect of raising the temperature of the higher air above what it would have been had the rate of decrease continued uniformly from the earth upwards.

There are several instances of a second or even a third *sudden* fall in the dew-point, but any corresponding variation in the temperature is not so clearly exhibited, probably owing to the *total* amount of moisture in the air being, at low temperatures, so very small that even a considerable change in its *relative* amount would produce but a small thermal effect.

As the existence of the disturbance in the regular progression of temperature now stated rendered it necessary, in order to arrive at any approximate value of the normal rate of diminution with height, to make abstraction of the portion affected by the disturbing cause, each series was divided into two *sections*, the first comprising the space below the stratum in which the irregularity existed, and the second commencing from the point where the regular diminution of temperature was resumed. It was then found that the rate of diminution was nearly uniform within each *section*, but that it was somewhat greater in the lower than in the upper sections.

On taking a mean of both sections for each series, giving each section a value corresponding to its extent, it is found that the number of feet of height corresponding to a fall of one degree Fahrenheit is—

On August 17	292·0 feet.
August 26	290·7 ,,
October 21	291·4 ,,
November 10	312·0 ,,

The first three values being remarkably coincident, and the last differing from them by about $\frac{1}{15}$th of the whole.

The air collected in the ascents was analysed by Dr. Miller; he states that "the specimens of air do not differ in any important amount from that at the earth at the same time, but contain a trifle less oxygen. All of them contained a trace of carbonic acid, but the quantity was too small for accurate measurement upon the small amount of air collected."

June 2, 1853.

The EARL OF ROSSE, President, in the Chair.

The Annual General Meeting for the Election of Fellows was held.

The Statutes respecting the election of Fellows having been read, Admiral Sir Francis Beaufort, and James Walker, Esq., were, with the consent of the Society, appointed Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following Gentlemen were declared duly elected:—

James Apjohn, M.D.
John George Appold, Esq.
John Allan Broun, Esq.
Antoine Jean François Claudet, Esq.
Edward J. Cooper, Esq.
E. Frankland, Esq.
John Hall Gladstone, Esq.
Commander Inglefield, R.N.
Joseph Beete Jukes, Esq.
Robert MacAndrew, Esq.
Charles Manby, Esq.
Joseph Prestwich, Esq.
William John Macquorn Rankine, Esq.
William Wilson Saunders, Esq.
William Spottiswoode, Esq.
Count P. de Strzelecki.

The Society then adjourned.

June 9, 1853.

The EARL OF ROSSE, President, in the Chair.

The following Gentlemen were admitted into the Society:—

John George Appold, Esq.
Antoine François Jean Claudet, Esq.
Edward Frankland, Esq., Ph.D.
John Hall Gladstone, Esq., Ph.D.
Robert MacAndrew, Esq.
Charles Manby, Esq.
Count Strzelecki.

The following papers were read:—

1. "Further Experiments and Observations on the Properties of Light." By Lord Brougham, F.R.S., Member of the Institute of France. Received May 9, 1853.

1. The author considers that Sir Isaac Newton's experiments to prove that the fringes formed by inflexion and bordering the shadows of all bodies, are of different breadths when formed by the homogeneous rays of different kinds, are the foundation of his theory, and would be perfectly conclusive if the different rays were equally bent out of their course by inflexion, for in that case the line joining the centres of the fringes on opposite sides of the shadow being, as he found them, of different lengths, the fringes must be of different breadths. He found that line to be $\frac{1}{37\frac{1}{2}}$ inch in the red, $\frac{1}{46}$ in the violet of the nearest fringe; $\frac{1}{22}$ in the red, $\frac{1}{27}$ in the violet of the second fringe; and these proportions he found to be the same at all distances of the chart from the hair. But if the rays are of different flexibility, if the red, for example, is bent to a greater distance from its course than the violet, the experiment becomes wholly inconclusive; and the line joining the centres may be greater in the red than in the violet, although the breadths of the two fringes are equal, or even though the violet fringe may be broader than the red.

2. A variety of experiments are adduced in the paper to show that this property of different flexibility exists, which Sir I. Newton had

not remarked. These experiments are either made with two bodies acting jointly on the rays, or with a single body so acting.

3. When made with two bodies, as sharp edges, these edges must be perfectly parallel, and when placed in the axis of the prismatic spectrum they form fringes more distant in the red than in any other part; least distant in the violet. The fringes are both broadest in the least refrangible rays and most removed; narrowest and least removed in the most refrangible. They incline from the red towards the violet.

4. The same experiment is easily made with a lamp or candle, placing a prism between the flame and the edges. This renders that exact parallelism of the edges which is required in the experiment with the spectrum, comparatively immaterial; because a considerable inclination of the edges, as at an angle of half a degree or more, does not affect the action on the rays in the very small space through which they pass by the edges.

5. With a single edge, or other body as a hair, the same difference in the position, as well as in the breadth of the fringes, is found to be observable, though not so manifestly as when two act together on the light. The manner of making the observation most conveniently is pointed out.

6. These experiments are varied so as to show the various distensions of the disc of a flame subjected to flexion between two edges, according as we regard the various portions of the flame's spectrum when seen by the prism. The red part is broadest, and when the near approach of the edges to each other divides the disc into two with a dark interval between them, that interval is the broadest in the least refrangible rays.

7. The experiments are further varied by using coloured glass instead of refracting with a prism.

8. The same phenomena are found to exist in all the other cases of flexion as where it is combined with reflexion by the action of specula, or by the effect of striated surfaces. There is always the same difference in the effects produced by the different kinds of homogeneous light.

9. The same phenomena are not so easily observed in the internal fringes, or those of the shadow; but the dark gray line in the axis of the shadow, having a line of deep black on each side, is found to vary in breadth and position in the different parts of its length corresponding to the colours of the spectrum, when a needle or other small body is placed in that spectrum.

10. The angle of inflexion is shown to be obtained by taking the distance at which the internal fringes begin to appear; and the proportion of this angle in the red to the same angle in the violet is ascertained. The deflexion (as deduced from Sir I. Newton's experiments) is much greater than inflexion appears to be. He had not observed the internal fringes of Grimaldi, to whom, however, he refers.

11. The author states that the property in question, the different flexibility of light, coexists with the other property, whatever it may

be, which disposes the different rays in fringes of different breadths; but he considers that the two properties are wholly independent of each other.

12. He thinks there is reason to believe that the dark intervals between the fringes made in white light are only the dark tint of the adjoining fringes, of which the red of one runs into the violet of the other. The greatest care in repeating Sir I. Newton's experiment, with the same distances and sizes both of the body and the beam, leaves little or no doubt of the fringes running into each other. In homogeneous light it is otherwise; and there appear in that case to be the intervals, as might be expected from the different flexibility of the different rays.

13. The fringes made in homogeneous light have a considerable admixture of colours from the scattered rays; so have the small spectra by refraction made when a second prism is placed behind a small hole in the screen, through which hole the rays of the spectrum made by the first prism are successively passed.

14. The phenomena of flexion by bodies placed in the portion of the spectrum near the prism, and therefore white, are stated to be not easily accounted for in any received theory.

15. The Newtonian hypothesis of the different breadths of the fringes being caused by the action of flexion extending to different distances over the different rays, is stated to be insufficient to account for it, and also to account for the different colours in the fringes to be made by white light. It is considered that the different flexibility will account for the latter, but not for the different breadths of the fringes, without another hypothesis, namely, the different ratio of the force to the distance from the bending body, in different rays.

16. The entire difference of flexion and refraction is shown from the different breadths of the fringes, and from their formation upon any possible hypothesis being shown to have nothing similar or analogous in the phenomena of refraction, though the different flexibility is precisely similar to the different refrangibility, only applicable inversely to the different rays.

17. The relation of the doctrine of interference to the phenomena of flexion is considered; and it is shown that certain of these phenomena are at variance with the doctrine. This is particularly exemplified in the case of the phenomena observed where bodies acting on light are not placed directly opposite to each other, but one behind the other.

18. The same phenomena are adduced to disprove M. Fresnel's hypothesis, that the phenomena of flexion (termed by him diffraction) depend entirely on the size of the aperture through which the light enters. Three experiments are adduced in disproof of this; the *first* made on the aperture when the edges are directly opposite each other; the *second*, when the edges are moved to different distances from each other on a line exactly parallel to the rays; the *third*, when the edges are moved on a line at any inclination to the rays. In both the *second* and *third* experiment, the vertical distance of the edges (*i. e.* the aperture) being the same, the breadth as well as the

separation of the fringes is found to vary with the distance of the edges from each other horizontally, or in the direction of the rays.

2. "Researches on the distribution of the Blood-vessels, &c. in the Lungs." By James Newton Heale, M.D. Communicated by J. Hodgson, Esq., F.R.S. Received May 20, 1853.

After referring to the discrepancies in the opinions entertained by anatomical writers both with respect to the distribution and to the functions of the blood-vessels with which the lungs are supplied, the author states the leading features in which the observations made by him differ from those which have hitherto been published. He finds that:—

1st. The pulmonary artery makes no anastomosis whatever with any other artery, nor do its own branches anastomose together; its branches go direct to the air-cells, and are there distributed, and terminate as arteries; none of its branches go to any other tissues of the lungs besides the air-cells, except some few which perforate the sub-pleural cellular tissue, and are distributed to the pleura; some of these also cross the posterior mediastinum beneath the pleura, and reach the thoracic pleura.

2ndly. The bronchial (so called) arteries have their own special distribution, which will be described further on; they do *not* supply, in the smallest degree, any portion of the *bronchial mucous membrane*, and they form no sort of communication, either with the pulmonary arteries or veins, except as supplying their cellular sheaths, and therefore in all probability furnishing their *vasa vasorum*.

3rdly. The bronchial mucous membrane is very freely supplied with an exceedingly vascular plexus, of a peculiar and very characteristic description, which is found to ramify in every part of the bronchial membrane, and which may be traced even as high as the trachea. The whole of this plexus is derived from the *air-cells*, and terminates ultimately by means of minute radicles, which form trunks and join the *pulmonary veins*. No trace whatever of any branches of the pulmonary artery, previous to this becoming capillary in the air-cells, is found in any part of the bronchial membrane.

4thly. The blood being brought to the air-cells by means of the pulmonary artery, is wholly returned by the pulmonary veins; but the trunks of these latter are formed by the junction of two distinct sets of radicles, namely, one set which comprises those which are formed from the perimeters of the air-cells (*i.e.* that part of the air-cell which is distant from the bronchial tube to which it is connected), and which at once form trunks which are visible on the surface of the lungs, and of all the lobules, and especially of the surfaces which adjoin the interlobular fissures; the other set consist of those which are derived from the bases of the air-cells, and which supply the bronchial membrane, and then terminate by radicles forming trunks, which join the before-mentioned set of pulmonary veins; and from these conjoined, the larger venous trunks are derived, which at length accompany the larger bronchi, and the pulmonary arteries, and which finally terminate in the left auricle of the heart; so that

the blood brought to the left auricle is formed of that portion which comes direct from the air-cells, and that which, after leaving the air-cells, has undergone the further process of distribution on the bronchial membrane, and has been the source from whence the epithelium and the bronchial mucus have been derived, and may therefore be considered as somewhat altered in character from that which, coming direct from the air-cells, has not been exposed to such changes.

5thly. It is possible to inject entirely the pulmonary artery and veins, without injecting the bronchial artery or veins; and it is also possible thoroughly to inject the latter without at all injecting the former; and when that is the case, *i. e.* the (so called) bronchial arteries are injected while the pulmonary vessels are empty, it will be found that the bronchial membrane is wholly uninjected, however perfectly the (so called) bronchial vessels may have been filled.

6thly. By injecting the lung through the pulmonary veins, the bronchial membrane becomes thoroughly injected, even before the air-cells are so; on the contrary, when the pulmonary artery is alone injected, the air-cells become injected long before the liquid reaches the bronchial membrane. In neither of these cases are the bronchial arteries, *i. e.* those derived from the aorta or veins which correspond to them, in the slightest degree injected.

7thly. The bronchial arteries, which are injected by filling the *aorta*, terminate in veins, which ramify in the subpleural cellular tissue; the greater part of these, after ramifying on the surface of the lung beneath the pleura, pass along the broad band of pleura, which extends from the peduncle of the lung to the posterior mediastinum, and encloses the root of the lung, and which may be called the mesopleura, and the veins then empty themselves into the œsophageal veins and other veins in the posterior mediastinum. It is probable also that some terminate in the azygos veins, the jugular veins, the diaphragmatic veins, and the venæ cavæ; in short, wherever they can meet with a systemic vein situated conveniently; but they form no sort of communication with the pulmonary veins, either in their capillaries or their larger trunks.

8thly. It is found that the coats of the lymphatic vessels of the lung are supplied by blood-vessels which are derived from the air-cells, and which terminate in the pulmonary veins, and the distribution of the blood-vessels on the coats of the lymphatics bears a strong resemblance to those distributed on the bronchial membrane.

The author then describes his apparatus for injection; the subjects injected; and the drawings which accompany his communication. In conclusion he states, that sufficient has been adduced to confute the opinion that there is one set of vessels for the nutrition of the lung in its ordinary acceptation, and another for the respiratory function. Without doubt the bronchial (so-called) vessels and the pulmonary are distinct, both as to their distribution and functions; the one being for the purposes of breathing, while the other solely supplies the cellular tissue of the organ.

3. "Theory of the reciprocal Action between the Solar Rays and the different Media by which they are reflected, refracted or absorbed." By Joseph Power, Esq., M.A., Fellow of Clare Hall and Librarian of the University of Cambridge, &c. Communicated by the Rev. Jonathan Cape, M.A., F.R.S. Received May 26, 1853.

For the train of thought which suggested the considerations in this communication, the author states that he is more particularly indebted to the researches of Professor Draper of New York, contained in his work "On the Organization of Plants, the Chemical effects of the Solar Rays," &c., his experiments tending to show that the law of action and reaction which prevails so generally in other departments of nature is no less true in all the varied phenomena of the sun-beam, so that the latter cannot be reflected, refracted, much less absorbed, without producing some effect upon the recipient medium. Whilst however he acknowledges the information he has received from that work, he differs in opinion with its author, as to the necessity of admitting more than one imponderable, being strongly of opinion that all the effects of the solar rays may be attributed to some or other of the infinite variety of undulations of which the universal ether is capable, and which, in the case of the sun-beam, are impressed upon it at the surface of the sun. He considers that the *vis viva*, which has its origin in these vibrations, is transmitted through the ether, with the velocity of light, in extremely minute undulations of different lengths and periods. If then a sunbeam, fraught with a vast variety of such undulations, be incident upon a medium so constituted that particles are capable of vibrating in unison, or even in harmonious consonance less perfect than unison, with some or other of the ethereal vibrations of the incident beam, it must necessarily happen that one system of vibrations will be called into existence by the other according to the laws of *Resonance*. He states that there may be a difficulty in explaining, but there can be no doubt of the fact, that the *vis viva* due to such induced vibrations may, like those of heat, become more or less persistent in the medium, producing at one time the phenomenon of fixed chemical action; at another that of permanently latent heat; at another that of less permanently latent or retarded heat; at another that of coloration and absorption; at another that of phosphogenic action. The remarkable phenomena lately discovered by Professor Stokes seem to him to be closely allied to the latter, differing however in the circumstance that they cease to exist the moment the exciting rays are withdrawn. Guided by analogy, he is, however, inclined to think that these phenomena will be found hereafter to possess some slight though insensible duration, while he regards all action which is really momentary as expending itself upon the passing rays as they emerge in the form of reflected or refracted rays.

But all these effects, of whatever kind, the author regards as due to one and the same cause, which can be no other than the expenditure or distribution of the *vis viva* originally derived from the sun, and conveyed by the ether. Such expenditure he considers we may regard as of two kinds, according as the *vis viva* is retained by the

medium, or transmitted with the emergent rays. If it be expended solely upon the emergent rays, the *vis viva* of the incident ray ought to be exactly equal to the sum of the *vires vivæ* of the reflected and refracted rays. But if it be partly expended on the medium and partly upon the emergent rays, the *vis viva* of the incident ray ought to exceed the *vires vivæ* of the reflected and refracted rays by a certain quantity. The object of his present investigation was to take into account the effect of such supposed excess, in the hope of arriving at some explanation of the Stokesian phenomena. The remarkable result he has obtained, that *every loss of* vis viva *will be accompanied with a diminution of the refractive index*, is quite in the direction of Professor Stokes's own idea of "a change of refrangibility," but throws no light on the change of *period*. This the author is inclined to think is due to an action of the nature of harmonic resonance, and from some calculations which he has made, he thinks it probable that the light produced in the Stokesian experiments may be due to resonant vibrations which are about a major or minor third lower in pitch than those of the invisible rays producing them.

The mode of procedure which appeared to the author most likely to lead to a successful result, was to assume, in the first instance, the hypothesis that the *vis viva* is expended solely on the reflected and refracted rays, and afterwards to modify, if possible, the steps of the process so as to adapt them to the hypothesis that a portion of it is expended on the medium. In adopting the more simple hypothesis, he was much struck by the formulæ at which he arrived; for not only did the general law of refraction spring out most unexpectedly, but the very same expressions for the intensity of the reflected rays, which were first discovered by Fresnel, and subsequently verified by the experiments of Brewster and Arago, were an immediate consequence of the formulæ. His results however differ in some particulars from those of Fresnel. In the first place, the index of refraction is not the simple quotient of the velocities of undulation, but of those velocities each multiplied by the density of the ether in the corresponding medium. In the second place, the vibrations of the ethereal particles are performed *in* the plane of polarization, and *not perpendicular* to that plane, as Fresnel supposed. Further, the expressions for the intensities of the refracted rays differ slightly in other respects from those of Fresnel, as given in Airy's Tracts.

The author states that he confines his attention to an isotropical singly refracting medium, though he thinks, if he had more time at his disposal, he could extend the theory to doubly refracting crystals. A very simple integration gives him a general expression for the *vis viva* of an elementary cycloidal wave, in terms of the amplitude and the constants of the periodical function. By help of this he obtains two equations of *vis viva*; one for a wave whose vibrations are in the planes of incidence, and the other for a wave whose vibrations are perpendicular to that plane, both vibrations being transverse to the axis of the ray. By the principle of superposition these two equations will hold true simultaneously when the above waves are

regarded as the components of one and the same wave. He obtains three other equations between the amplitudes from the simple consideration that a particle situated in the common surface of the two media cannot vibrate in more than one way at once. Of these three equations two involve the amplitudes of the first component wave, and the third those of the second. The five equations serve to determine, in terms of the angle of incidence and the component amplitudes of the incident wave, the five following quantities, namely, the angle of refraction, the two component amplitudes of the reflected wave, and those of the refracted wave.

By the aid of the result referred to, that every loss of *vis viva* is always accompanied by a diminution of the refractive index, coupled with the general view which he takes of the cause of absorption, the author is enabled to give an explanation of Frauenhofer's lines, and in general of the lines of absorption in coloured media; and also to explain the phenomena discovered by Sir David Brewster, that violet light may exist in the blue spaces, and blue light in the red.

In the course of the investigation, the altered expressions for the intensities of the reflected and refracted rays, so far as they are affected by the coefficient of absorption, are given. The expressions for the intensities of the two component reflected waves are very little affected; but those for the intensities of the two component refracted waves are materially altered in value.

The theory likewise affords an explanation, the first the author believes that has ever been offered, of the remarkable properties of saccharine solutions and of certain crystals, such as right-handed and left-handed quartz, which exhibit the phenomenon of circular polarization.

June 16, 1853.

The EARL OF ROSSE, President, in the Chair.

The following gentlemen were admitted into the Society:—

Joseph Prestwich, Esq.; William Wilson Saunders, Esq.; and William Spottiswoode, Esq.

The following papers were read:—

1. "On the Anatomy and Physiology of *Cordylophora*, a contribution to our knowledge of the Tubularian Zoophytes." By George James Allman, M.D., M.R.I.A., Professor of Botany in the University of Dublin, &c. Communicated by Professor Edward Forbes, F.R.S. &c. Received May 31, 1853.

The author, after pointing out the necessity of giving greater definiteness to the terminology employed in the description of the true zoophytes, proceeds to the anatomical details of *Cordylophora*, a genus of *Tubulariadæ*. He demonstrates that *Cordylophora* is essentially composed in all its parts of two distinct membranes enclosing a cavity, a structure which is common to all the *Hydroida*. For greater precision in description, he finds it necessary to give to these

membranes special names, and he therefore employs for the external the name of *ectoderm*, and for the internal that of *endoderm*. Each of these membranes retains its primitive cellular structure. In the ectoderm *thread-cells* are produced in great abundance; these are formed in the interior of the ectodermal cells by a process of endogenous cell-formation, and are afterwards set free by the rupture of the mother-cell. The thread-cells in a quiescent state are minute ovoid capsules, but under the influence of irritation, an internal sac is protruded by a process of evagination; the surface of the evaginated sac is furnished with a circle of curved spicula, and from its free extremity a delicate and long filament is emitted. The thread-cells of *Cordylophora* thus closely resemble the "hastigerous organs" of *Hydra*. The polypary is a simple unorganized secretion deposited in layers from the ectoderm. In the endoderm, the author points out a distinct and well-developed glandular structure composed of true secreting cells, which are themselves produced in the interior of mother-cells, and elaborate a brown granular secretion which he assumes as representing the biliary secretion of the higher animals. He describes, as a system of special muscles, certain longitudinal fibres, which may be distinctly seen in close connection with the inner surface of the ectoderm. The tentacula are shown to be continuous tubes communicating with the cavity of the stomach, and thus possess the same essential structure as those of *Hydra*; they are formed of a direct continuation of the ectodern of the polype, lined by a similar continuation of the endoderm. The appearance of transverse septa at regular intervals, which is so very striking in these tentacula, must not be attributed to the existence of true septa. It is due to a peculiar condition of the endodermal layer, but the author has not been able to give a satisfactory explanation of it. Through the whole of the canal which pervades the axis of the stems and branches, a constant though a regular rotatory movement is kept up in the contained fluid; this movement is not due to the propulsive action of vibratile cilia, and is explained by the author as the effect of the active processes going on in the secreting cells of the endoderm, processes which can scarcely be imagined to take place without causing *local* alterations in the chemical constitution of the surrounding fluid, and a consequent disturbance in its stability.

The reproductive system of *Cordylophora* consists of ovoid capsules situated on the ultimate branches at some distance behind the polypes; some of these capsules contain *ova*, others *spermatozoa*; they are plainly homologous with the ovigerous sacs of the marine *Tubulariadæ*; they present a very evident, though disguised medusoid structure, having a hollow cylindrical body, whose cavity is continuous with that of the polype-stem, projecting into them below, and representing the proboscidiform stomach of a Medusa, while a system of branched tubes which communicate at their origin with the cavity of the hollow organ, must be viewed as the homologues of the radiating gastro-vascular canals, and the proper walls of the capsule will then represent the disc. From comparative observations

made on other genera of *Hydroida*, the author maintains the presence of a true medusoid structure in the fixed ovigerous vesicles of all the genera he has examined, and he arrives at the generalization, that for the production of true ova in the hydroid zoophytes, a particular form of zooid is necessary, in which the ordinary polype-structure becomes modified, and presents, instead, a more or less obvious medusoid conformation, *Hydra* being at present the only genus which appears to offer an exception to this law, though the author believes that the exception is only apparent, and that further observations will enable us to refer the reproductive organization of this zoophyte to the same type with that of *Cordylophora* and the marine *Hydroida*. The author has satisfied himself that the ova-like bodies contained in the capsules of *Cordylophora* are true *ova*, and not *gemmæ*; he has demonstrated in them a distinct germinal vesicle, and has witnessed the phenomenon of yelk-cleavage; and the paper details the development of the embryo to the period of its escape from the capsule in the form of a free-swimming ciliated animacule, and traces its subsequent progress into the condition of the adult zoophyte.

2. "On the Secular Variation of the Moon's Mean Motion." By J. C. Adams, Esq., M.A., F.R.S. &c. Received June 16, 1853.

The author remarks, that in treating a great problem of approximation, such as that presented to us by the investigation of the moon's motion, experience shows that nothing is more easy than to neglect, on account of their apparent insignificance, considerations which ultimately prove to be of the greatest importance. One instance of this occurs with reference to the secular acceleration of the moon's mean motion. Although this acceleration and the diminution of the eccentricity of the earth's orbit, on which it depends, had been made known by observation as separate facts, yet many of the first geometers altogether failed to trace any connexion between them, and it was not until he had made repeated attempts to explain the phenomenon by other means, that Laplace himself succeeded in referring it to its true cause.

The accurate determination of the amount of the acceleration is a matter of very great importance. The effect on the moon's place, of an error in any of the periodic inequalities, is always confined within certain limits, and takes place alternately in opposite directions within very moderate intervals of time, whereas the effect of an error in the acceleration goes on increasing for an almost indefinite period, so as to render it impossible to connect observations made at very distant times.

In the 'Mécanique Céleste,' the approximation to the value of the acceleration is confined to the principal term, but in the theories of Damoiseau and Plana, the developments are carried to an immense extent, particularly in the latter, where the multiplier of the change in the square of the eccentricity of the earth's orbit, which occurs in the expression of the secular acceleration, is given to terms of the seventh order.

As these theories agree in principle, and only differ slightly in the numerical value which they assign to the acceleration, and as they passed under the examination of Laplace, with especial reference to this subject, it might be supposed that only some small numerical rectifications would be required in order to obtain a very exact determination of this value.

It has not been, therefore, without surprise, which he has no doubt will be shared by the Society, that the author has lately found that Laplace's explanation of the phenomenon in question is essentially incomplete, and that the numerical results of Damoiseau's and Plana's theories, with reference to it, consequently require to be very sensibly altered.

Laplace's explanation may be briefly stated as follows. He shows that the mean central disturbing force of the sun, by which the moon's gravity towards the earth is diminished, depends not only on the sun's mean distance, but also on the eccentricity of the earth's orbit. Now this eccentricity is at present (and for many ages has been) diminishing, while the mean distance remains unaltered. In consequence of this, the mean disturbing force is also diminishing, and therefore the moon's gravity towards the earth at a given distance is, on the whole, increasing. Also the area described in a given time by the moon about the earth is not affected by this alteration of the central force; whence it readily follows that the moon's mean distance from the earth will be diminished in the same ratio as the force at a given distance is increased, and the mean angular motion will be increased in double the same ratio.

This, the author states, is the main principle of Laplace's analytical method, in which he is followed by Damoiseau and Plana; but it will be observed that this reasoning supposes that the area described by the moon in a given time is not permanently altered, or in other words, that the tangential disturbing force produces no permanent effect. On examination, however, he remarks it will be found that this is not strictly true, and he proceeds briefly to point out the manner in which the inequalities of the moon's motion are modified by a gradual change of the disturbing force, so as to give rise to such an alteration of the areal velocity.

As an example, he takes the case of the *variation*, the most direct effect of the disturbing force. In the ordinary theory, the orbit of the moon, as affected by this inequality only, would be symmetrical with respect to the line of conjunction with the sun, and the areal velocity generated while the moon was moving from quadrature to syzygy, would be exactly destroyed while it was moving from syzygy to quadrature, so that no permanent alteration would be produced.

In reality, however, the magnitude of the disturbing force by which this inequality is caused, depends in some degree on the eccentricity of the earth's orbit; and as this is continually diminishing, the disturbing forces at equal intervals before and after conjunction will not be exactly equal. Hence the orbit will no longer be symmetrically situated with respect to the line of conjunction, and there-

fore the effects of the tangential force before and after conjunction no longer exactly balance each other.

The other inequalities of the moon's motion will be similarly modified, especially those which depend, more directly, on the eccentricity of the earth's orbit, so that each of them will give rise to an uncompensated change of the areal velocity, and all of these must be combined in order to ascertain the total effect.

Since the distortion of the orbit just pointed out is due to the change of the disturbing force consequent upon a change in the eccentricity of the earth's orbit, and the action of the tangential force permanently to change the rate of description of areas, is only brought into play by means of this distortion, it follows that the alteration of the areal velocity will be of the order of the square of the disturbing force multiplied by the rate of change of the square of the eccentricity. It is evident that this alteration of areal velocity will have a direct effect in changing the acceleration of the moon's mean motion.

Having thus briefly indicated the way in which the effect now treated of originates, the author proceeds with the analytical investigation of its amount. In the present communication, however, he proposes to confine his attention to the principal term of the change thus produced in the acceleration of the moon's mean motion, deferring to another, though he hopes not a distant opportunity, the fuller treatment of this subject, as well as the determination of the secular variations of the other elements of the moon's motion, which, arising from the same cause, have also been hitherto overlooked.

In the usual theory, the reciprocal of the moon's radius vector is expressed by means of a series of *cosines* of angles formed by combinations of multiples of the mean angular distance of the moon from the sun, of the mean anomalies of the moon and sun, and of the moon's mean distance from the node; and the moon's longitude is expressed by means of a series of *sines* of the same angles, the coefficients of the periodic terms being functions of the ratio of the sun's mean motion to that of the moon, of the eccentricities of the two orbits and of their mutual inclination.

Now, if the eccentricity of the earth's orbit be supposed to remain constant, this is the true form of the expressions for the moon's coordinates; but if that eccentricity be variable, the author shows that the differential equations cannot be satisfied without adding to the expression for the reciprocal of the radius vector, a series of small supplementary terms depending on the *sines* of the angles whose *cosines* are already involved in it, and to the expression for the longitude, a series of similar terms depending on the *cosines* of the same angles; all the coefficients of these new terms containing as a factor the differential coefficient of the eccentricity of the earth's orbit taken with respect to the time.

The author first determines as many of these terms as are necessary in the order of approximation to which he restricts himself, and then takes them into account in the investigation of the secular ac-

celeration. The expression which he thus obtains for the first two terms of this acceleration, is

$$-\left(\frac{3}{2}m^2-\frac{3771}{64}m^4\right)\int(e'^2-E'^2)ndt.$$

According to Plana, the corresponding expression is

$$-\left(\frac{3}{2}m^2-\frac{2187}{128}m^4\right)\int(e'^2-E'^2)ndt.$$

It will be observed that the coefficient of the second term has been completely altered in consequence of the introduction of the new terms.

The numerical effect of this alteration is to diminish by 1″·66 the coefficient of the square of the time in the expression for the secular acceleration; the time being, as usual, expressed in centuries.

It will, of course, be necessary to carry the approximation much further in order to obtain such a value of this coefficient as may be employed with confidence in the calculation of ancient eclipses.

In conclusion, the author states, that the existence of the new terms in the expression of the moon's coordinates occurred to him some time since, when he was engaged in thinking over a new method of treating the lunar theory, though he did not then perceive their important bearing on the secular equation. His attention was first directed to this subject while endeavouring to supply an omission in the Theory of the Moon given by Pontécoulant in his 'Théorie Analytique.' In this valuable work, the author, following the example originally set by Sir J. Lubbock in his tracts on the lunar theory, obtains directly the expressions for the moon's coordinates in terms of the time, which are found in Plana's theory by means of the reversion of series. With respect to the secular acceleration of the mean motion, however, Pontécoulant unfortunately adopts Plana's result without examination. On performing the calculation requisite to complete this part of the theory, the author was surprised to find that the second term of the expression for the secular acceleration thus obtained, not only differed totally in magnitude from the corresponding term given by Plana, but was even of a contrary sign. His previous researches, however, immediately led him to suspect what was the origin of this discordance, and when both processes were corrected by taking into account the new terms whose existence he had already recognized, he had the satisfaction of finding a perfect agreement between the results.

3. "On a Theory of the conjugate relations of two rational integral functions, comprising an application to the Theory of Sturm's Functions, and that of the greatest Algebraical Common Measure." By J. J. Sylvester, Esq., M.A., F.R.S., Barrister at Law. Received June 16, 1853.

The memoir consists of four sections. In the first section, the theory of the residues obtained by applying the process of the common measure to two algebraical functions is discussed. It is shown that

a certain superfluous or *allotrious* factor enters into each, the value of which, in terms of the leading coefficients of the residues in their simplified form, is determined; and the simplified residues themselves are subsequently obtained from the given functions by a direct method.

In the case where the two functions are of the same degree (m) in x, m functions of the degree $m-1$ in x are formed, which, being identical with those employed in the process which goes by the name of Bezout's abridged method, the author terms the Bezoutics or Bezoutic primaries. By linear elimination performed between these, a second system of functions, whose degrees in x extend from $m-1$ to 0, are formed, which he terms the Bezoutic secondaries: these Bezoutic secondaries are proved to be identical with the simplified residues. A similar theory is shown to be applicable in the general case of the functions being of unlike degrees. Other modes of obtaining the simplified residues by a direct method are also given. The coefficients of the primary system of Bezoutics form a square symmetrical about one axis, to which (as to every symmetrical matrix) a certain homogeneous quadratic function of (m) variables is appurtenant. This quadratic function is termed the Bezoutiant, the properties of which are discussed in the fourth section.

Every residue is what may be termed a syzygetic function or conjunctive of the two given functions; these being respectively multiplied by certain appropriate rational integral functions, their sum may be made to represent a residue. These multipliers are termed the syzygetic multipliers; and they form two series, one corresponding to the successive numerators, the other to the successive denominators of the convergents to the algebraical continued fraction which expresses the ratio of the two given functions. The residues are obviously a particular class of the conjunctives that can be formed from the given functions; every conjunctive has the property of vanishing when the two functions to which it is appurtenant vanish simultaneously; and in general, for any given degree in x, an infinite number of such conjunctives can be formed.

In the second section, the author commences with obtaining in terms of the roots and factors of the two given functions, a variety of forms, all containing *arbitrary* forms of function in their several terms, and representing a conjunctive of any degree not exceeding the sum of the degrees of the two given functions in its most general form. The author then reverts to the Bezoutic system of the first section, and obtains the general solution for the conjunctive of any given degree in x in terms of the *coefficients* of the given function; by aid of this general solution he demonstrates that the residues obtained by the common measure process (divested of their allotrious factors), are the conjunctives of the lowest *weight* in the roots of the given functions for their several degrees; and obtains the value of this weight. He then demonstrates that certain rational but fractional forms ascribed to the arbitrary functions in the general expressions for a conjunctive in terms of the roots, will make these expressions integral and of the minimum weight; they will all be

consequently identical (save as to a numerical factor) with one another, and with the simplified residues. The formulæ thus obtained for the simplified residues deserve particular attention on their own account, being double sums of terms, any single series of which is made up of fractions whose denominators are the products of the differences between a certain number of the roots of each one of the functions and a certain other number of the same combined in every possible manner, thus containing a vast extension of the ordinary theory of partial fractions. The author subsequently determines under a similar form, the value of each of the multipliers which connects the given functions syzygetically with the simplified residues, and establishes a general theorem of reciprocity, by aid of certain general properties of continued fractions, between the series of residues and either series of syzygetic multipliers.

The third section is divided into two parts. The first part is devoted to a determination of the values of the preceding formulæ in the case to which Sturm's theorem refers, where one of the given functions is the first differential derivative of the other; when this is the case the roots and factors of the second function are functions of those of the first, and it will be found that one of the polymorphic representations for the residue of any given degree will consist of terms, each of which is convertible into an integral function of the roots and factors of the given primitive function; in this way are obtained the author's well-known formulæ for Sturm's auxiliary functions. In like manner, the multiplier which affects the derivative function in the syzygy between the primitive, the derivative and any simplified residue, may also be expressed immediately as a sum of integral functions of the roots and factors of the primitive, complementary in some sort to the formulæ for the residues. The formula for the remaining syzygetic multiplier, (that which attaches to the primitive itself,) cannot be obtained directly by a similar method, but it is deduced by aid of the syzygetic equation itself, all the other of the five terms of which are known, or have been previously determined. The process of obtaining this last-named multiplier is one of great peculiarity and interest, and results in a form far more complex than that for the residues or for the other syzygetic multiplier.

In the second part of the third section are contained some curious and valuable expressions for the residues and multipliers, communicated to the author by M. Hermite; and an instantaneous demonstration is given of the properties of the author's formulæ for Sturm's auxiliary functions in determining the real roots of an equation by a method quite irrespective of the theory of the common measure, and depending upon a certain extremely simple but unobserved law of quadratic forms, which he terms the law of *inertia*. In place of these formulæ it is shown that others greatly more general, and possessing the same properties as regards the determination of the real roots, may be substituted; the known formulæ are, however, the most simple that can be employed. The author then proceeds to inquire as to the nature of the indications afforded by the signs of a series of successive simplified residues, taken between any two functions

independent of one another, instead of standing in the relation of primitive and derivative, as in Sturm's theorem; this leads to the theory of interpositions, of whch it is shown that the Sturmian theorem may be treated (not so much as a particular case) as an easy corollary. In this part, the author obtains an entirely new rule for determining, in an infinite variety of ways, a superior and inferior limit to the real roots of any algebraical equation, whether numerical or literal.

The fourth section is also divided into two parts. In the first part, the index of interposition for two functions of the same degree is shown to be determinable by means of the quadratic form, previously termed the Bezoutiant; and as a corollary, it follows that the number of real roots of an equation of the degree m depends in a direct manner on the number of positive roots in another equation of the degree $m-1$, all of whose roots are real, and the coefficients of which are quadratic combinations of the coefficients of the given equation.

In the second part of this section, the Bezoutiant is considered under a purely morphological point of view. It is shown to be a combinantive invariant of the two given functions (each treated as homogeneous functions of two variables), remaining unaltered when any linear combination of the two given functions is substituted for the functions themselves, and also when any linear substitutions are impressed upon the variables of the given functions, provided that certain corresponding substitutions are impressed upon the variables of the Bezoutiant. The family of forms to which the Bezoutiant belongs is ascertained, and a method given for finding the constituent forms of this family (one less in number than the number of odd integers not exceeding in magnitude the degree of either of the given functions which, throughout this section, are supposed to be of equal dimensions in x), of which all other forms of the family will be numerico-linear functions. The numerical coefficients connecting the Bezoutiant with this constituent group, are calculated for the cases corresponding to any index from 1 to 6 in clusive. Finally, the author remarks upon the different directions in which the subject matter of the ideas involved in Sturm's justly celebrated theorem admits of being expanded, and of which the most promising is, in his opinion, that which leads through the theory of interpositions. Several of the theorems in this memoir have been previously published by the author, but they are here given along with a great deal of new matter in a connected form, and with the demonstrations annexed, for the first time.

4. "On the frequent occurrence of Indigo in Human Urine, and on its Chemical and Physiological Relations." By Arthur Hill Hassall, M.D., Physician to the Royal Free Hospital, &c. Communicated by Professor Sharpey, F.R.S. Received June 9, 1853.

The author was led to the investigations laid before the Society in the above communication, by the following circumstances:—

Some three or four years since, when examining urinary deposits

under the microscope, he frequently observed in the field of vision particles of a deep blue colour; so often did this occur, that he could not even then help suspecting that their presence was not accidental; however, no analysis of the blue colouring matter was made at that time, and the circumstance was in a fair way of being forgotten, until the recollection of it was revived by another occurrence.

In June 1852, a sample of urine, freely exposed to the air in an open vessel, was observed after four or five days' exposure, gradually to change colour; the pellicle or scum which had formed upon the surface of the urine became first slate-coloured, and at length deep blue, with here and there a rusty-red tint. The urine also underwent at the same time some remarkable changes; it became thick and turbid, deep brown, greenish, bluish-green, and finally of a faded yellowish-green colour; a considerable sediment was found at the bottom of the glass; this was deep brown, intermixed with a little blue colouring matter, and it had a medicinal smell resembling somewhat that of *Valerian*. In this state, without undergoing any further material changes, the urine remained for many days.

Examined with the microscope, the scum or pellicle on the surface was found to consist of vibriones, innumerable animalcules, and crystals of triple phosphate, with a great many fragments and granules of a deep and bright blue colour.

A second sample of the same urine was therefore procured, taking every precaution to avoid fallacy. Gradually the same changes ensued as in the first sample, and this likewise became blue. Having thus ascertained that the changes observed were due to something contained in the urine itself, the author next proceeded to set aside in open vessels, a series of urines all from the same patient, noting the alterations which occurred from day to day; these samples underwent nearly similar changes; but the quantity of blue colouring matter and brown extractive gradually diminished, until at length they were present in such small amount as to be visible only under the microscope, and at last they entirely disappeared.

The results obtained from an examination of the *urine*, the *blue colouring matter* and the *brown extractive*, are then given by the author; they are as follow:—

The urine.—The urine of the second sample at the time of analysis, when shaken, had a dark, greenish-brown colour; but on standing at rest for some time, the colouring matter fell to the bottom, forming bluish-green flocculi, while the supernatant liquid was of a deep wine-red colour. The bottle was set aside corked, for ten days, at the end of which time the bluish-green precipitate had entirely disappeared; but on removing the cork, and allowing free access of air for some time, the coloured deposit was again produced. This was washed with water, drenched with hydrochloric acid, and finally dried; by this means was obtained a rich blue powder possessing all the chemical characters and properties of indigo.

The urine that was filtered off from the above precipitate was allowed to evaporate spontaneously, by which means it yielded an

additional quantity of indigo, which adhered in the form of very small flakes to the sides of the dish. It also gave a rather large proportion of a deliquescent brown colouring matter, which was treated, first with alcohol, and then with water.

The alcohol acquired a deep brownish-red colour, and the water a dark brownish-green. Both of these solutions were evaporated at a temperature of 160° Fahr. The alcoholic solution furnished a rich brown extractive which was soluble in water, but not in dilute acids, and nitric acid did not produce that play of colours which is characteristic of bile pigment; nor did the precipitate formed with basic acetate of lead furnish a purple liquid with alcohol and free acid. A strong solution of potash dissolved the extractive and yielded a deep blood-red fluid, which was rendered green and opalescent by boiling. These reactions show that the brown pigment was somewhat like hæmatin in its chemical manifestations.

While the aqueous solution of the brown matter was undergoing evaporation, it gave a further supply of indigo, which was formed most freely at the edge of the liquid. The residue was made black by concentrated sulphuric acid and deep brown by potash.

The blue colouring matter.—Of two samples of this in a dry state, mixed with a large quantity of earthy phosphates, vibriones, mucus, and epithelium, one gave a dark brown solution with concentrated sulphuric acid, and the other a dirty blue. Both of these solutions were decomposed by water, furnishing in the former case a dark brown deposit, and in the latter a dirty green. In most of their other reactions, however, they presented the characters of indigo; and it is especially deserving of notice, that they were reduced by lime and sugar, giving a liquid from which hydrochloric acid threw down a greenish-blue precipitate.

The cause of concentrated sulphuric acid giving with one of these samples a brown solution, and with the other only a dirty blue, was, the author considers, mainly owing to the large quantity of animal matter with which the specimens were contaminated; the acid, from its charring effect on this, would produce a brown or blackish solution, thus obscuring the colour of the solution of indigo.

The brown extractive.—The brown extractive yielded nearly the same results as on its first analysis, and the aqueous solution furnished a few blue flocculi. A portion of the alcoholic extract was heated with Liq. Potassæ for the purpose of ascertaining whether it contained leucine, and the product, on being treated with hydrochloric acid, gave off a powerful odour, which was somewhat like *valerianic acid*; but the result was too doubtful to be of much value. The author had already referred to the peculiar smell of *Valerian* emitted by the extractive of more than one of the samples. He considers that the clearest and most positive evidence was thus obtained that the blue colouring matter in this case was indigo.

It was not very long after the occurrence of the first case of blue urine that numerous other instances fell under the author's observation. The urines of all these cases underwent very nearly the same changes as in the first; in some, the quantity of blue colouring mat-

ter found was very considerable, in others less; and in the third class of cases the microscope was necessary for its discovery. In nearly all these cases the blue colouring matter was submitted to analysis, and ascertained, on the clearest evidence, to be indigo.

The author in the next place considers the question of the source and origin of indigo in the urine.

It appeared that in the cases related by the author, coloured indigo was not present in urine when first voided, but that it was gradually formed some time afterwards by a process of oxidation on exposure to the air, being in most of the cases probably derived from the brown extractive, which in its chemical manifestations so closely resembles hæmatin.

The author contrasts *cyanourine* with the indigo detected in urine. He observes that the most distinctive tests laid down for cyanourine are its solubility in boiling alcohol, and the action of sulphuric acid upon it, which give a reddish-brown solution; and states he had ascertained that these tests are not to be relied upon, since indigo, when contaminated (as in the urine it frequently is) with a large quantity of animal matter, vibriones, &c., gives a reddish-brown solution with concentrated sulphuric acid, from the charring of the animal matter, and in many cases forms a bright blue solution with boiling absolute alcohol; hence he could not help suspecting that cyanourine and indigo are very closely connected with each other, if they be not identical. He observes, it is at least singular that while so many cases of indigo were met with, not a single instance of cyanourine presented itself. He also contrasts indigo with apoglaucin, and remarks that this is acknowledged by Heller himself to be nothing more than cyanourine mixed with urrhodin.

Taking then into consideration the whole of the facts described in this communication, the following conclusions are deduced:—

1st. That blue indigo is frequently formed in human urine, the quantity being subject to the greatest variation; in some cases it is so considerable as to impart a deep green, or bluish-green colour to the whole urine; a pellicle of nearly pure indigo also extending over the entire surface of the liquid; while in others it is so small that it can only be detected by means of the microscope.

2nd. That for the formation of this indigo, it is in general necessary that the urine should be exposed to the air for some days in an open vessel, oxygen being absorbed and the blue indigo developed. Whatever facilitates therefore oxygenation, as free exposure to light and air, warmth and sunshine, hastens the appearance of the blue indigo; hence in summer the changes described take place much more quickly than in winter; on the contrary, these changes are retarded and even stayed by exclusion of the atmosphere. Blue indigo may even be deprived of its colour and reformed, alternately, according as air is excluded or admitted to urine containing it. From some of the cases recorded, it would appear, however, that blue indigo is occasionally formed in the system, and is voided as such in the urine.

3rd. That there is usually found with the blue indigo, where the

amount of this is considerable, a brown extractive, sometimes in large quantity, the aqueous solution of which, by exposure to air, yields a further supply of coloured indigo, and which closely resembles hæmatin in its chemical manifestations and elementary composition. There is therefore great reason for believing that in the majority of the cases here recorded, the blue indigo was derived from altered hæmatin, although it is at the same time probable, that in some cases it is formed from modified urine pigment which is itself supposed to be a modification of hæmatin. Between the greater number of the animal colouring matters there is the closest relationship in chemical composition, so that the transformation of the one into the other would appear to be both easy and natural.

4thly. That the urines in which the coloured indigo occurs in the largest quantity, are usually of a pale straw colour, readily becoming turbid, alkaline, and of low specific gravity. Small quantities of indigo are however frequently found in urines possessing characters the very reverse, that is, in such as are high-coloured, acid, and of high specific gravity; but, as a rule, in these urines the blue pigment is usually absent.

5thly. That as coloured indigo does not occur in healthy urine, and since where the amount of this is at all considerable it is accompanied with strongly-marked symptoms of deranged health, the formation of blue indigo in urine must be regarded as a strictly pathological phenomenon, apparently associated rather with some general morbid condition, than essentially with disease of any one organ; although there is reason for believing that the blue deposit is met with very frequently in Bright's disease, and in affections of the organs of respiration, it should however be remarked that none of the worst cases of indigo in the urine which the author met with were cases of Bright's disease.

The paper is illustrated by drawings, and a specimen of the indigo, as deposited from urine, was exhibited.

5. "On the Thermal Effects of Elastic Fluids." By Professor William Thomson, F.R.S., and J. P. Joule, Esq., F.R.S. Received June 15, 1853.

The authors had already proved by experiments conducted on a small scale, that when dry atmospheric air, exposed to pressure, is made to percolate a plug of non-conducting porous material, a depression of temperature takes place increasing in some proportion with the pressure of the air in the receiver. The numerous sources of error which were to be apprehended in experiments of this kind conducted on a small scale, induced the authors to apply for the means of executing them on a larger scale; and the present paper contains the introductory part of their researches with apparatus furnished by the Royal Society, comprising a force pump worked by a steam-engine and capable of propelling 250 cubic inches of air per second, and a series of tubes by which the elastic fluid is conveyed through a bath of water, by which its temperature is regulated, a flange at the terminal permitting the attachment of any nozle which is desired.

Preliminary experiments were made in order to illustrate the thermal phenomena which result from the rush of air through a single aperture. Two effects were anticipated, one of heat arising from the *vis viva* of air in rapid motion, the other of cold arising from dilatation of the gas and the consequent conversion of heat into mechanical effect. The latter was exhibited by placing the bulb of a very small thermometer close to a small orifice through which dry atmospheric air, confined under a pressure of 8 atmospheres, was permitted to escape. In this case the thermometer was depressed 13° Cent. below the temperature of the bath. The former effect was exhibited by causing the stream of air as it issued from the orifice to pass in a very narrow stream between the bulb of the thermometer and a piece of gutta percha tube in which the latter was enclosed. In this experiment, with a pressure of 8 atmospheres, an elevation of temperature equal to 23° Cent. was observed. The same phenomenon was even more strikingly exhibited by pinching the rushing stream with the finger and thumb, the heat resulting therefrom being insupportable.

The varied effects thus exhibited in the "rapids" neutralize one another at a short distance from the orifice, leaving however a small cooling effect, to ascertain the law of which and its amount for various gases, the present researches have principally been instituted. A plug of cotton wool was employed, for the purpose at once of preventing the escape of thermal effect in the rapids, and of mechanical effect in the shape of sound. With this arrangement a depression of $0^{\circ}{\cdot}31$ Cent. was observed, the temperature of the dry atmospheric air in the receiver being $14^{\circ}{\cdot}5$ Cent., and its pressure 34·4 lbs. on the square inch, and the pressure of the atmosphere being 14·7 lbs. per square inch.

Erratum.—In Mr. Joule's letter to Col. Sabine, "Proceedings of the Royal Society," p. 307, line 27, *for* 2·67 *read* 0·267.

6. "On Clairaut's Theorem and Subjects connected with it." By Matthew Collins, Esq., B.A., Senior Moderator in Mathematics and Physics of Trin. Coll. Dublin. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S. &c. Received May 2, 1853.

The author begins his investigations by proving the existence of principal axes for any point of a body, which he makes to depend on the existence of principal axes of an auxiliary ellipsoid (Poinsot's central one) having its centre at the given point, and such that any semidiameter of it is reciprocally proportional to the radius of gyration of the body about that semidiameter.

He afterwards employs another ellipsoid (called McCullagh's ellipsoid of inertia) concentric to the former and reciprocal to it, which admirably suits and facilitates the remainder of his investigations, and whose characteristic property is this, that it gives the radius of gyration itself (and not its reciprocal, as in Poinsot's) about any semidiameter of it, the radius of gyration being in fact equal to the portion of that semidiameter between the centre and a tangent plane perpendicular to it.

He then proves that the attraction of a body of any shape, whose

centre of gravity is O and mass is μ, on a very remote point P along $PO=d$, is $\frac{\mu}{d^2}+\frac{3}{2d^4}(A+B+C-3M)$, A, B, C being the three principal moments of inertia of the body, and M its moment about OP. And if MCullagh's ellipsoid of inertia be taken having O its centre, and its principal axes coinciding in direction with the principal axes of the body at O; and if a tangent plane to this ellipsoid perpendicular to OP at P′ touch it in R, it is shown that the component of the attraction of the body μ on P in a direction perpendicular to OP is parallel to RP′, and equal to $\frac{3\mu}{d^4}\times OP'\times P'R$.

Next comes the proposition, "if two confocal ellipsoids attract an external point, their two resultants are coincident in direction and proportional to their masses," the truth of which is very easily inferred from Ivory's theorem. This proposition is then employed in proving that the expressions already found for the attractions of a body of *any shape* on a very remote point hold true likewise for the attractions of *an ellipsoid* (whether it be homogeneous, or only composed of concentric ellipsoidal strata having the same principal axes, and any variable but small excentricities) on any external point, whether near or remote.

To apply these reasonings to the case of the earth, the ellipsoid is then supposed to become a spheroid, and the attracted point P is supposed on its surface; then $C=B$ and $M=B\cos^2\lambda+A\sin^2\lambda$, λ being the angle $OP(=d)$ makes with the equator; and so the central attraction along PO, viz. $\frac{\mu}{d^2}+\frac{3}{2d^4}(A+B+C-3M)$, then becomes $\frac{\mu}{d^2}+T(1-3\sin^2\lambda)$, where $T=\frac{3}{2d^4}(A-B)$: the attraction of the spheroid on P perpendicular to PO and urging P towards the equator is also easily shown to become $T\sin 2\lambda$.

Now that the point P may be at rest, it is necessary that the tangential component of the central force acting along PO should be equal to the sum of the tangential components of the centrifugal force (acting on P perpendicular to the earth's axis), and of the force perpendicular to PO; this condition gives an equation from which Clairaut's theorem follows instanter, due regard being had to the difference of the polar and equatorial gravities as determined by the general expression $\frac{\mu}{d^2}+T(1-3\sin^2\lambda)$, and the ellipticity of the exterior surface being supposed so small that its square and higher powers may be rejected.

7. "On the Change of Refrangibility of Light."—No. II. By Professor Stokes, M.A., F.R.S. Received June 16, 1853.

The principal object of this paper is to explain a mode of observation by means of which the author found that he could exhibit, with ordinary day-light, the change of refrangibility produced by substances opake as well as transparent, even when they possessed

only a low degree of sensibility. The method requires hardly any apparatus; it is extremely easy in execution; and it has the great advantage of rendering the observer independent of sun-light. On these accounts the author conceives that it might be immediately applied by chemists to the discrimination between different substances. The method is as follows:—

A large hole, which ought to be several inches in diameter, cut in the window-shutter of a darkened room, serves to introduce the light, and a small shelf, blackened on the top, attached to the shutter immediately underneath the hole, serves to support the objects to be examined, as well as one or two absorbing media. The hole is covered by an absorbing medium, called by the author the *principal absorbent*, which is so selected as to let through, as far as may be, the feebly illuminating rays of high refrangibility, as well as the invisible rays still more refrangible, but to stop the rays belonging to the greater part of the visible spectrum. A second medium, called by the author the *complementary absorbent*, is chosen so as to be as far as possible transparent with regard to those rays which the first medium stops, and opake with regard to those which it lets through. The object to be examined is placed on the shelf, and viewed through the second medium. If the media be well-selected, they together produce a very fair approach towards perfect darkness; and if the object appears unduly luminous, that arises in all probability from "fluorescence." To determine whether the illumination be really due to that cause, it is commonly sufficient to remove the complementary absorbent from before the eyes to the front of the hole, when the illumination, if it be really due to fluorescence, almost wholly disappears; whereas, if it be due merely to scattered light which is able to get through both media, it necessarily remains the same as before. In the case of objects which are only feebly fluorescent, it is sometimes better to leave the second medium in its place, and use a third medium, called by the author the *transfer medium*, which is placed alternately in the path of the rays incident on the object-end of the rays coming from it to the eyes.

Independently of illumination, the change of colour corresponding to the change of refrangibility, and the difference of colour with which the object appears, according as the transfer medium, or the complementary absorbent used as a transfer medium, is held in front of the eyes or in front of the hole, afford in most cases a ready mode of detecting fluorescence.

Instead of trusting to the *absolute* appearance of the object, it is commonly better to compare it with some fixed standard. The standard substance ought to be such as to scatter freely visible rays of all refrangibilities, but not to give out rays of one refrangibility when influenced by rays of another. The author employed a white porcelain tablet as such a standard; and the object to be observed was placed on the tablet, instead of being laid directly on the blackened shelf.

Another mode of observation consists in using a prism in combination with the principal absorbent. The object being placed on

the tablet, a slit is held close to it, in such a position as to be seen, projected partly on the object and partly on the tablet, and the slit is then viewed through a prism. The fluorescence of the object is evidenced by light appearing in regions of the spectrum, in which, in the case of the rays coming through the principal absorbent, and, therefore, in the case of the rays scattered by the tablet, there is nothing but darkness.

The author states that these methods proved to be of such delicacy, that, even on an unusually gloomy day, he was able readily to detect the fluorescence of white paper; and even in the case of substances standing much lower in the scale, the fluorescence could be detected in a similar manner.

In conclusion, the author states that he had found the property of fluorescence to belong to a peculiar class of salts, the platinocyanides, making a third instance in which this property had been connected with substances chemically isolated in a perfectly satisfactory way. The present instance opens a new field of inquiry in relation to the polarization of the fluorescent light.

8. "Researches in Embryology; a Note supplementary to Papers published in the Philosophical Transactions for 1838, 1839 and 1840, showing the confirmation of the principal facts there recorded, and pointing out a correspondence between certain structures connected with the Mammiferous Ovum and other Ova." By Martin Barry, M.D., F.R.S., F.R.S.E. Received May 27, 1853.

Referring to his account of the process of fecundation of the mammalian ovum and the immediately succeeding phenomena, published in various papers in the Philosophical Transactions, the author calls attention to the confirmation which his views have received from corresponding observations made by subsequent inquirers on the ova of other animals. He more particularly adverts to a recently published memoir by Dr. Keber, in which that physiologist describes the penetration of the spermatozoa into the interior of the ovum, in *Unio* and *Anodonta*, through an aperture formed by dehiscence of its coats, analogous to the micropyle in plants.

Small pellucid vesicles, lined with ciliated epithelium and enclosing a revolving mulberry-like object, such as the author discovered imbedded under the mucous membrane of the rabbit's uterus and described in the Philosophical Transactions for 1839, have been likewise observed by Keber, not only under the mucous membrane, but also and most frequently in some part of the cavity of the abdomen. Keber considers these bodies to be fecundated ova. The author agrees with Keber in considering them to be ova, but he does not suppose them to be fecundated, nor does he think that their membrane is the vitellary membrane ("zona pellucida"), which he believes to have been absorbed. He considers such ova to have been detached from the ovary along with their containing ovisac, which in their new situation constitutes the ciliated capsule, and as they present themselves in unimpregnated animals, he now believes that the formation of a mulberry-like group of cells from the germinal

spot and the process of division and subdivision of the latter take place without fecundation; but when this happens, the mulberry is not found to contain one cell larger than the rest, the nucleus of which, according to his observations, is the embryo. He is further of opinion, that in all cases of separation of ova, the ovisac or internal coat of the Graafian follicle is detached from the ovary, either entire and along with the ovum, as in the instances alluded to, or after the ovum has first escaped by rupture, as in the instance of the fecundated ovum.

The author is led to the following conclusions with reference to the structures connected with the ovum in different animals:—1. That in the mammalia the vesicle he described as the foundation of the Graafian follicle, and termed the ovisac, *does not remain permanently in the ovary,* but is *expelled and absorbed.* 2. That in the Bird, the ovum, when escaping from the ovary, is accompanied by the corresponding vesicle—the ovisac; and that *the ovisac becomes the shell-membrane of the Bird's egg.* 3. That the expelled and lost ovisac in the mammalia therefore corresponds to the shell-membrane in the Bird. 4. That after the formation of the ovum the albuminous contents of the ovisac in the mammalia correspond to the albumen in the Bird's egg. 5. That the author's retinacula in the mammalia, after all, find their analogue in the chalazæ of the Bird; and that both have their origin in the granular contents of the ovisac, which at an early period are in appearance just the same in both. 6. That the shell-membrane of the Bird is thus a primary cell.

He then points out the position which from his observations is to be assigned to the several parts of the ovum in the language of "cells;" and shows the presence of a plurality of ova in a Graafian follicle to be referable to the same cause as that producing more than one yelk (ovum) in the Bird's egg.

The Society then adjourned over the Vacation to Thursday, November 17, 1853.

November 17th, 1853.

Dr. WALLICH, V.P., in the Chair.

Dr. Booth gave notice that at the next meeting of the Society he would propose the Earl of Harrowby for immediate ballot as a Fellow of the Royal Society, to which as a Peer of the Realm his Lordship is entitled.

The following papers were read:

1. "On the Nerves which supply the Muscular Structure of the Heart." By Robert Lee, M.D., F.R.S. Received Nov. 7.

The author remarks that, in a paper entitled "On the Ganglia and Nerves of the Heart," published in the Philosophical Transactions, Part I. 1839, it is asserted, that "it can be clearly demonstrated that every artery distributed throughout the walls of the uterus and heart, and every muscular fasciculus of these organs, is supplied with nerves upon which ganglia are formed."

He then states that "recent dissections which I have made of the heart of the race-horse, in which both the muscular and nervous structures are largely developed, demonstrate, that from the outer surface to the lining membrane the walls are universally pervaded with nerves, on which ganglia are formed, or enlargements invested with neurilemma, into which nerves enter and from which they issue, as in all the other ganglia of the great sympathetic nerve.

"From these dissections it is seen that the ganglionic nerves which ramify on the surface of the heart, those which have hitherto been delineated in the works of anatomists, are few in number compared to those which are distributed throughout the muscular structure of the organ, many of which are wholly independent of the blood-vessels.

"This anatomical demonstration of the ganglia and nerves of the muscular structure of the heart, completely subverts the opinion still entertained by some physiologists, that the sensitive and contractile powers of the heart are independent of nervous influence. It further indicates the real source of the action of the heart as an entire organ, from the commencement to the termination of life; how the circulation of the blood is carried on when the fœtus has neither brain nor spinal cord, and how the detached parts of the heart continue to contract for a time in some animals after its total separation from the body.

"These dissections are now open to examination by any gentleman who may feel interested in the anatomy and physiology of the heart.

"When Mr. West's drawings of the nerves displayed in these dissections have been completed, they will be presented to the Royal Society, with a description of the appearances delineated."

2. "On the Influence of the Moon on the magnetic direction at Toronto, St. Helena, and Hobarton." By Colonel Edward Sabine, R.A., V.P. and Treas. Received Nov. 17, 1853.

Having noticed the inference drawn by M. Kreil from the magnetic observations at Milan and Prague, that the moon exercises an influence on the magnetic direction at the surface of our globe, cognisable by a variation in the declination depending on the moon's hour-angle and completing its period in a lunar day, the author proceeds in this paper to state the results of an examination, analogous to that pursued by M. Kreil, into the influence of the moon on the magnetic declination at the three stations of Toronto, St. Helena, and Hobarton.

The observations employed in this investigation consisted of six years of hourly observation at Toronto, five years at St. Helena, and five years at Hobarton, forming, exclusive of observations omitted on account of excessive disturbance, a total of 105,747 observations.

The processes are related by which, after the separation of the disturbances of largest amount, the observations were treated, for the purpose of eliminating the variations due to solar influence, and of re-arrangement in a form by which the inequality of the moon's action at the different hours of each lunar day might be brought distinctly into view. The results are shown in tables exhibiting the amount of inequality at each of the three stations corresponding to each of the twenty-four lunar hours.

It appears from these results that the existence of a lunar diurnal variation in the magnetic declination is shown at each of the three stations of Toronto, St. Helena, and Hobarton, and that it has the same general character at each, viz. that of a double progression in a lunar day, having two easterly maxima nearly at opposite points of the hour-circle, and two westerly maxima also at nearly two opposite points of the hour-circle. The extreme elongations are not at precisely opposite points of the hour-circle at any of the three stations, nor have the amounts of the two elongations which take place in the same direction always precisely the same value; but the slight inequalities in these respects are within the limits which might be ascribed to accidental variations, and might therefore disappear with longer continued observations. It is otherwise, however, in the author's opinion, with the disparity between the amounts of easterly and westerly extreme elongations which presents itself at each of the three stations. At Hobarton and St. Helena the westerly elongations have the larger values, at Toronto the easterly (the north end of the magnet being referred to in all cases).

The times at which the extreme elongations in the two directions take place are not the same at the three stations, and are as follows:—At Toronto the easterly extremes take place about the hours of 0 and 12, being the hours of the upper and lower culminations; at St. Helena the westerly extremes about two hours before the culminations; and at Hobarton about two hours after the culminations. At Toronto the westerly extremes take place about the hours of 6 and 18; at St. Helena and Hobarton the easterly extremes respect-

ively two hours before and two hours after the same hours of 6 and 18. The extreme inequality, or the amount of lunar variation measured from one extreme elongation to the other, is about 28″ of arc at Toronto, 20″ at Hobarton, and 11″ at St. Helena. The resolved portion of the terrestrial magnetic force which acts in the horizontal direction, and is opposed to any disturbing influence, is approximately 3·54 at Toronto, 4·51 at Hobarton, and 5·57 at St. Helena.

November 24, 1853.

The EARL OF ROSSE, President, in the Chair.

The Earl of Harrowby was elected.

Captain Inglefield, R.N., was admitted into the Society.

The following gentlemen were elected Auditors of the Treasurer's Accounts on the part of the Society:—Capt. FitzRoy, Mr. Grove, Mr. Phillips, The Rev. Baden Powell, Mr. Wheatstone.

The following extract of a letter from Lieut. Gilliss, U.S.N. to Colonel Sabine, R.A., was read:—

Washington, 12 Sept. 1853.

Lieut. Mackai returned to the United States in April, having made his magnetical observations successfully at all the elevations and at distances of 100 miles, entirely across the Pampas. Soon after leaving Mendoza he was thrown from his horse, breaking his barometer and so injuring his chronometer, that he has neither the longitudes of his magnetical stations nor barometric profile of the country. Being desirous to make his work complete, he volunteered to retrace his ground, and left the United States for the purpose more than a month ago, taking with him the declinometer and dip-circle, two Bunten's barometers, an apparatus for determining altitudes from the boiling-point, and some smaller instruments.

Conveyance of the unifilar would have required another mule.

Cursory inspection of the observations already made afforded evidence of their reliability, and as repetition would have involved many hours' detention at each station, it was not considered essential to cumber him. Should the Argentine provinces have become sufficiently quiet, he will first cross the Andes at the Planchan Pass, lat. 35° 20′, next at the Partillo (the most elevated) Pass, lat. 33° 40′, and finally at the Cumbre and Uspalata Pass, in lat. 32° 50′, where observations have already been made.

As he will remain at Santiago only a short time, I look for him home during February next.

His and all the magnetical observations will then be discussed, and the volume be ready for press by the close of 1854. Those on meteorology are very far advanced.

The astronomical observations will fill three volumes, one of which (Mars and Venus) will be ready about the same time as the magnetical and meterological volume; the zones not until two years later.

My report, embracing those of Lieut. Mackai and the naturalists, with the maps, plans, and drawings, will probably be the first published; and should Congress extend its usual liberality to my propositions, all the volumes will be presented to the world in a creditable manner.

A paper was also read, entitled "On the Typical Forms of the large Secreting Organs of the Human Body." By Thomas H. Silvester, M.D. Communicated by Thomas Bell, Esq., Sec. R.S. Received Nov. 11, 1853.

The author in this paper offers some observations on the large secreting organs of the human body, namely,—

The lungs,
The liver,
The kidney,
The stomach,
The intestines, and
The developments connected with reproduction.

He observes that there are great obstacles to an attempt to reduce them to a uniform type, but that further observation reveals a typical uniformity, in adaptation to special purposes, unequalled in the rest of the human œconomy. The greatest hindrance to the discovery of this law of formation has been the habit of regarding a single element of the glandular organ as the whole and not a mere part of the glandular apparatus; for instance, the ovary has been described as the ovuliferous gland, although in reality the latter consists of the ovary, the Fallopian tubes, the uterus, and the vagina: also the lung has been said to be a gland, meaning by this expression the secreting portion; whereas the lung-gland is composed of the vesicular structure, the excretory ducts, the bronchi, larynx, thyroid body, and even the nasal passages. The aim of the author has been to discover the constant elements of the glandular system and to compare them together in the several organs.

A secreting gland is described as a more or less developed fold of mucous membrane, or even a plain surface--in its simplest form a follicle; but in the organs to be described it is a highly complicated apparatus, composed of many parts, each possessing a peculiar function. It is an unvarying characteristic of these structures that the elements of which they are composed always bear the same relation to each other in position. An accompanying diagram exhibited the type of a perfect gland, constituted of five elements and an appendage.

1. The secreting element.................... Red.
2. The excretory element.................... Blue.
3. The receptacle element .... Yellow,
4. The cervix with its glandular appendage .... Purple.
5. The efferent duct........................ Green.

Sketches of the several glands of the human body accompanied the paper, coloured in the same manner, so that the corresponding parts might be seen at a glance. The elements are seldom wanting,

though sometimes scarcely discernible in their rudimentary condition, and retained apparently only in conformity to typical law.

There is great difference in degree of development of the elements. The secreting part of the liver is of large size compared to the ducts, whilst that of the generative organ in the female, namely, the ovarium, is comparatively minute. The appendage of the cervix is very large in the biliferous apparatus, but scarcely discoverable in the stomach and sigmoid flexure. The excretory duct in the digestive organ, represented by a contraction across the middle of the stomach, is of enormous length in the seminiferous gland, as the vas deferens.

The popular terms being inexact or without meaning, it was proposed to substitute for the ordinary names of the secreting organs designations more aptly descriptive of their functions and more in accordance with the principles of scientific nomenclature, as follows:—

The uriniferous gland.
The seminiferous gland.
The biliferous gland.
The pneumatiferous gland.
The intestinal gland.
The ovuliferous gland.
The lactiferous gland; and
The digestive gland.

The kidney, or uriniferous gland, presents an almost typical regularity of development. The secreting element or cortical portion is composed of small convoluted tubes covered with a net-work of blood-vessels.

The excretory duct or medullary structure arises from the cortical part by numerous straight tubuli, which terminate in a duct for the conveyance of the urine into

The receptacle or bladder. This organ has a cervix lined with mucous membrane in longitudinal folds, studded with minute follicles and a rudimentary glandular appendage in the male.

The efferent duct is constituted of the membranous portion of the urethra.

The seminiferous gland consists of the testicle, a collection of convoluted tubercles, of an excretory duct called the vas deferens, of a receptacle the analogue of the uterus, the vesiculæ seminales being the body, the prostate the glandular appendage surrounding the cervix. The efferent duct is of considerable length; the receptacle is lined with an alveolar fissure like the gall-bladder and some muscular fibres.

The ovuliferous organ consists of the ovarium or secretory element, the fallopian tubes or excretory duct, the uterus or receptacle, the cervix and its glandular appendage in the shape of mucous follices, and the vagina or efferent duct. The secretory structure is not tubular, but vesicular; the excretory duct is connected with the ovarium by its fimbriated extremity at certain periods only, in conformity with its peculiar function.

The lactiferous gland is remarkable for the singular distribution

of its elements; the secreting portion or breast, and the excretory duct or nipple, being attached to one individual, and the receptacle or mouth, cervix, glandular appendage or tonsils, and efferent duct or œsophagus belonging to another, of which the mother and child afford an illustration.

The stomach or digestive gland does not appear at first sight to be formed upon the same type as the other glands; it is however divisible into three distinct cavities—the secreting or cardiac, the receptacular or pyloric, and the efferent or duodenum. It is a tube of enormous calibre, divided into three compartments of unequal area. The excretory tube is recognized in the central contraction of the muscles during digestion; the cervix, lined with plicated mucous membrane, has been poetically described as the pylorus.

Comparative anatomy as well as microscopical bear testimony to the correctness of this view with regard to structure and development.

The biliferous gland is constituted as follows:

The secreting element, popularly called the liver.

The hepatic duct is the excretory tube.

The gall-bladder is the receptacle terminating in the duodenum in conjunction with the pancreatic duct, the pancreas being the glandular appendage surrounding the slender cervix of the receptacle, and the duodenum performing the function of an efferent canal to the digestive and biliferous glands.

The glandular appendages are subject to great variety of development. The thyroid, the pancreas, and the prostate are large structures, whilst in the pylorus, the sigmoid flexure of the colon and the cervix uteri, the same element is little more than a fold of mucous membrane with follicles interspersed. This element, like the glandular apparatus, is greatly predisposed to cancerous degeneration.

The writer of the paper now proceeds to describe the most remarkable and important gland of the human œconomy, namely, the sanguiferous.

The jejunum and ileum constitute the secreting element of this organ. The ileo-cæcal valve or verminiferous appendage is the excretory duct in a rudimentary state. The colon, though enormously developed in a longitudinal direction, must be regarded as the receptacle; the sigmoid flexure folded upon itself like the letter S (as in the cervix of the gall-bladder) is the cervix of the colon receptacle; the rectum or the efferent duct completes the system.

The function of this system is not fully known; the tract is lined with mucous membrane and glandulæ for the secretion of air and other products of the blood. It can scarcely be denied that the intestinal tube, in its structure and form and arrangement of the elements, bears the closest analogy to the glandular apparatus as seen in the other large viscera of the human body.

The jejunum, as its name implies, is generally empty; it is convoluted, and its parietes are covered with a net-work of capillaries; it resembles therefore in all these particulars the tubes which form

the secreting tissue of the testicle and kidney, and differs from them but in size. The jejunum and ileum, regarded as a continuous tube, constitute a magnificent secreting structure, and its function is probably something more than the supply of the air and mucus generally contained in its canal. It has been suggested, that the blood, the lymph and the fæces owe their existence to the active capillaries of the mesenteric arteries, but some certain proofs are still wanting in confirmation of this opinion.

In the lung-gland, or pneumatiferous organ, the vesicular structure is the secreting element; it secretes carbonic acid: the ramifications of the bronchia ending in the two bronchi represent the excretory duct; the trachea is the receptacle; and the glandular appendage covering the cervix (or cricoid cartilage) and marking its position, passes by the name of the thyroid.

The efferent duct commences at the cricoid and ends at the cartilaginous orifice of the nostrils.

The lacrymiferous organ is formed upon the same plan as the preceding; but in consequence of some peculiarities of development, that, together with the simpler secreting structures, will be reserved for a future opportunity.

November 30, 1853.

At the Anniversary Meeting,

The EARL OF ROSSE, K.P., M.A., President, in the Chair.

Mr. Grove, on the part of the Auditors of the Treasurer's Accounts, announced that the total receipts, during the past year, including a balance of £182 0*s.* 1*d.* carried from the account of the preceding year, amounted to £4791 12*s.* 1*d.*; and that the total expenditure, including an investment of £1367 10*s.* 0*d.* in the Funds, was £3784 14*s.* 6*d.*, leaving a balance in the hands of the Treasurer of £1006 17*s.* 7*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

List of Fellows deceased since the last Anniversary.

On the Home List.

The Earl of Brownlow.
Admiral Sir George Cockburn.
Col. Colquhoun.
Bransby Blake Cooper, Esq.
Sir George Gibbes.
Thomas Gordon, Esq.
Robert James Graves, M.D.
Rev. Henry Hasted.
John Hawkins, Esq.
Col. Jackson.
Captain Johnson, R.N.
The Lord Bishop of Lincoln.
Captain Napier, R.N.
Edward William Pendarves, Esq.
Jonathan Pereira, M.D.
The Dean of Peterborough.
Henry Barne Sawbridge, Esq.
George Poulett Scrope, Esq.
Sir William Smith.
John Robert Stewart, Esq.
Lieut. Stratford, R.N.
Hugh Edwin Strickland, Esq.
Rt. Hon. Sir Edward Thornton.
Rev. Charles Turnor.
Frederick Webb, Esq.

On the Foreign List.

François Jean Dominique Arago. | Baron Leopold von Buch.

List of Fellows elected since the last Anniversary.

On the Home List.

James Apjohn, M.D.
John George Appold, Esq.
John Allan Broun, Esq.
Antoine Jean François Claudet, Esq.
Edward J. Cooper, Esq.
E. Frankland, Esq.
John Hall Gladstone, Esq.
The Earl of Granville.
The Earl of Harrowby.
Captain Edward Inglefield, R.N.
Joseph Beete Jukes, Esq.
Robert MacAndrew, Esq.
Charles Manby, Esq.
The Viscount Palmerston.
Joseph Prestwich, Esq.
William John Macquorn Rankine, Esq.
William Wilson Saunders, Esq.
William Spottiswoode, Esq.
Count P. de Strzelecki.

The President then addressed the Society as follows :

Gentlemen,

I am happy that it is in my power again to congratulate you on the progress made in the researches which have been carried on, aided by the Grant the Government have placed at your disposal : after an experience of more than three years we may say with confidence that much has been accomplished. At first there were some misgivings : continental experience was not altogether relied upon. Although it had been very much the practice of foreign governments to take an active part in encouraging the pursuit of science, and with decided success, here the smallest effort in that direction was looked upon by some as an experiment little harmonizing with our institutions, our feelings, perhaps our prejudices, and only to be followed by failure and disappointment. It was feared that because occasionally there had been some difficulty in employing effectively the small fund which had been bequeathed to us, that therefore there would be increased difficulty in employing a larger fund ; and this no doubt would have happened if there had been restrictions limiting the application of the larger fund to certain specific objects, or if the field of discovery had been of limited extent : the reverse however was the case. Your Council were not embarrassed by any unwise restrictions, and in science there is room for every one. It is one of the deductions of economic science that labour creates a demand for labour ; in fact, that where a community is industrious, and labour accumulates and becomes capital, that there the people will be fully employed : the same is true in the inductive sciences, and it is true universally ; there no modifying causes interfere to diminish the force, or limit the application of the great principle, and we see strikingly that as facts accumulate, and facts are the capital of inductive science, fresh employment is everywhere provided for those who are willing to work. Take any one of the inductive

sciences as an example, and we at once see how this is. Take for instance chemistry, compare it as it now is with what it was when Priestley commenced his career. The whole of the science then consisted of an imperfect knowledge of the properties of a few of the metals, of sulphur, phosphorus, and the three alkalies as they were then called. There was a little known also about salts and acids, and the existence of hydrogen and carbonic acid gas had recently been ascertained. In a range so limited there was little room but for one master mind, when Priestley discovered oxygen, and at once an opening was made for researches into the nature of the atmosphere, of water, and of combustion, of the acids and the alkalies, and ample employment was provided for a host of distinguished philosophers for years to come. Other important discoveries were soon made, each becoming as it were a new origin of light, throwing perhaps at first but feeble rays upon the objects around us, but revealing so much of their strange forms as to excite curiosity, and awaken the strongest passion of the human mind,—the desire to discover the truth. Inorganic chemistry was then rapidly becoming a great science, when the foundations of organic chemistry were laid in a succession of brilliant discoveries. That was but a few years ago, but there were many men then ready trained for the work, and the progress was proportionally rapid. To take a few of the discoveries in organic chemistry, and show how each has been the germ of others, as it were the first term of a diverging series, and thus to exhibit the great principle at work that in science labour creates a demand for labour, might perhaps be of some interest, but it would lead me from the object which I have in view, which is simply to point out the grounds upon which I have ever felt a strong conviction, that whatever means were placed at the disposal of the Royal Society, no lasting difficulty could occur in turning them to useful account.

During the last year considerable progress has been made by Mr. Hopkins in the important experiments which he has been carrying on in conjunction with Mr. Fairbairn and Mr. Joule. You no doubt are aware, that, as we descend below the surface of the earth, it has been found that the temperature increases: numerous experiments made in different places with all the necessary precautions to guard against fallacy, seem clearly to have established the fact. The increase is about one degree of Fahrenheit for a depth of from 50 to 60 feet. If therefore the conducting power of the materials of the globe was the same at all depths, we should have a series, which would give us the depth proportional to every required temperature. Reasoning in this way, we conclude that a temperature higher than that of melting iron exists at a depth of thirty miles, and that at double that depth the materials of the surface of the globe, combined as we find them in nature, would enter into fusion. It has therefore been supposed by many that the solid crust beneath our feet is not more than forty or fifty miles thick. It has however been assumed that the increasing pressure at increasing depths does not alter either the conducting power of materials, or the temperature at which they melt. This no doubt is to a certain extent incorrect, and it is highly

probable that the conducting power of the different strata increases considerably with the depth, the materials becoming more compact under augmented pressure. It is not improbable also that pressure may raise the temperature of fluidity. In either case the solid crust of the globe would be thicker than it had been supposed to be on the assumed data. With the view of throwing light upon this question so interesting to all geologists, Mr. Hopkins undertook, with the assistance of Mr. Fairbairn and Mr. Joule, to subject various substances under different temperatures to enormous pressure. Considerable time was required, even with Mr. Fairbairn's unlimited mechanical means, to construct the necessary apparatus; however, recently it has been completed, and in the few substances examined it has been found that the temperature of fusion has increased with the pressure: in the case of wax, by a pressure of 13,000 pounds to the square inch, the fusing-point was raised 30°. Whatever may be the influence of these experiments as affecting the great questions of Terrestrial Physics, we may predict with certainty that data will be obtained most valuable in philosophical research.

Mr. Joule, I find, has been actively engaged, in conjunction with Professor Thomson, in his experiments on the thermal effects of fluids in motion, and has determined with considerable accuracy, operating on a great scale, the depression of temperature when compressed air escapes into the atmosphere through a porous plug. The laws of the phenomena as to the temperature and pressure of the confined gas, will also soon be determined.

Carbonic acid gas has been found to give a depression of four and a half times as great as atmospheric air, while it passes through the porous plug with greater facility than atmospheric air; equal volumes requiring pressures of 1 and 1·05 respectively in order to be transmitted in equal times. Certain heating effects of air rushing through a single orifice have been observed, which will probably lead to a further development of the mechanical theory of the temperature of elastic fluids in rapid motion.

The examination of the sedimentary deposits in the Nile valley, mentioned at the last Anniversary, is still going on. Mr. Horner states, that by the munificent aid of His Highness Abbas Pacha, the Viceroy of Egypt, a series of operations have been carried on at Heliopolis, and at another station thirteen miles above Cairo, which have led to interesting results. A pit has been sunk to the depth of 24 feet below the pedestal of the colossal statue of Ramses the Second, who reigned, according to the chronology of Bunsen, about 1400 years before Christ, and borings have been continued by which cylinders of soil have been extracted at an additional depth of 48 feet. A series of thirty-two pits has been sunk across the valley in a line between the Libyan and Arabian deserts, occupying a line of about five miles, passing through the site of the statue alluded to; and it is proposed to sink a similar line of pits next year about twenty miles lower down the river, passing through the site of the obelisk of Heliopolis. Above sixty persons were employed in the operations at Memphis. The plan, as proposed by Mr. Horner, was, through the

intervention of the Hon. Charles Augustus Murray, Her Majesty's late Consul-General in Egypt, submitted to the Viceroy, and met with the most ready acceptance. He gave directions to his government that every assistance should be afforded for carrying on the proposed researches; he appointed an able engineer officer high in his service, M. Hekekyan Bey, to conduct them, and ordered that the whole expense should be defrayed by his government. Such enlightened liberality on the part of His Highness Abbas Pacha justly entitles him to the gratitude of all cultivators of science.

The other researches alluded to on the last occasion are proceeding satisfactorily, but there is nothing which seems to call for especial notice at present; I will therefore at once proceed to give some account of the steps which have been taken by your Council for the advancement of science in another direction.

In the history of individual sciences we perceive there have been always successive periods of activity and repose. In Astronomy, for many years we have had a period of activity. Physical Astronomy has achieved perhaps its greatest triumph within the last few years in the discovery of Neptune; and the discovery of the numerous Asteroids and Comets is evidence that Practical Astronomy has kept pace with it. Within the same period the nebulous contents of the Southern Hemisphere have for the first time been made known to us; we have now a catalogue of the highest excellence, with an ample guarantee for its accuracy in the zeal, ability, and experience of Sir John Herschel.

That catalogue will be a record for future ages, leading probably to the detection of change in the wonderful objects revealed by the telescope, and so giving a clue to the mysterious laws which rule the remote universe.

The Cape observations were not long before the world when some astronomers expressed a desire that it should not be left to posterity to turn them to account, but that some effort should be made to employ them in the service of the present generation. It was suggested, that with such an admirable working list, much might be effected in a short time. A comparison of the Northern and Southern catalogues had led many to believe that the same instrument had effected more in the Southern than in the Northern Hemisphere. Whether that had been owing to a better atmosphere, or whether the objects themselves were more remarkable, in either case it was reasonable to expect that an instrument of great power would do more in a well-selected situation in the Southern Hemisphere than in these islands; and on that account alone there seemed to be grounds for a well-founded hope that interesting discoveries would be made; but there were other grounds.

In the present state of Nebular Astronomy, the best prospect we have of extending our knowledge, seems to be by carefully sketching and measuring every object sufficiently within reach of our instruments, to make details discernible. It is highly probable that the objects we see are presented to us in every variety of position, that they often differ in form merely because we see them in a different

aspect, and that if all were similarly placed as to the line of sight, a few normal forms would represent the whole. If this is the case, had we a sufficient number of accurate sketches, it is probable that out of the apparent confusion we should succeed in extracting the normal forms. It is also probable that in the nebular systems motion exists. If we see a system with a distinct spiral arrangement, all analogy leads us to conclude that there has been motion, and that if there has been motion that it still continues. The apparent motion is probably very slow, owing to the immense distance of the nebulæ; still there are double stars known to be physically double from their motions, which are probably as distant as some of the nebulæ. In certain nebulæ stars are so peculiarly situated that we can scarcely doubt their connection with the nebular system in which we see them, and some of these stars are as bright as some of the stars known to be physically double; as bright even as some of the stars which the latest Pulkowa observations have shown to have sensible parallax, and whose distance therefore is approximately known. We have therefore some vague idea, resting on probable evidence, even of the distance of the nearest nebulæ. It seems therefore not unreasonable to expect that measurements perseveringly carried on will detect motion, and that a fulcrum will thus be obtained, by which the powers of analysis may be brought to bear upon the laws which govern these mysterious systems.

In the northern catalogue of Nebulæ, and the same observation no doubt applies to the southern also, there are vast numbers much too faint to be sketched, or measured, with any prospect of advantage; the most powerful instruments we possess, showing in them nothing of an organized structure, but merely a confused mass of nebulosity of varying brightness. The number of nebulæ in the northern hemisphere, in which details are well brought out, is not very large, and even in these, a great proportion of the measurements are necessarily rough, and wanting in that precision by which the motion of certain double stars was so soon detected. It is evident therefore that to obtain a true knowledge of the forms of the nebular systems, our sketches must be as numerous as possible; and to obtain evidence of motion with rough measures, our measures should be very numerous also; but the northern hemisphere presents but a very limited list of suitable objects, therefore it is desirable to take in the southern hemisphere also.

I have thus, I believe, in part at least, explained the views of those who felt anxious that the southern hemisphere should be examined with a telescope of great power: the first attempt to give practical effect to their wishes was at the meeting of the British Association held at Birmingham in 1849.

The President of the Association was on that occasion directed to apply to Government to send a telescope of great power, in charge of an experienced observer, to the southern hemisphere. That application was unsuccessful; the Government, while they acknowledged the importance of the proposed object, declined to proceed, from an apprehension of difficulties, through which they did not then see

their way clearly. At a subsequent meeting of the British Association, the matter was again discussed, and a Committee was appointed, to take such measures as they considered best calculated to effect the object. The Committee, in the first instance, laid the whole matter before your Council, and the Council, approving of the suggestion, appointed a Committee to consider the subject in detail.

It was necessary before the Government could be applied to with a reasonable prospect of success, that a specific plan should be in readiness, complete in all its details: the size of the instrument, its optical principle, its mounting, and its site.

With the view of obtaining the best information, the Committee consulted several eminent men, conversant with the management of large instruments. The first question proposed was, whether the instrument should be a refractor or reflector, and it was decided in favour of the reflector—there was in fact no choice. In the present state of knowledge, there was no probability that a good refractor could be constructed of sufficient power. As to size, the Committee were disposed to recommend a telescope of 4 feet aperture, and 35 feet focal length, to be mounted equatorially; not that an instrument of that size was the best, a much larger no doubt would have been better, but it would have been obviously unwise to have recommended a plan involving a very large expenditure. The Committee proceeded one step further, they pointed out a mechanical engineer of character and experience in the construction of optical instruments, who was willing to undertake to make the instrument for a specific sum, and having proceeded thus far, they laid the whole matter before Government, with whom it at present rests.

Another application is also before the Government of a very different character, but with the same great object, the advancement of human knowledge: it is to provide a building for the reception of the Scientific Societies of the metropolis. That application did not proceed from your Council, but from a large number of individuals, many of them Fellows of this Society, the remainder distinguished members of the other Scientific bodies of the metropolis. The application was made in the shape of a Memorial to Government, the signatures amounting to about 200. There had been no effort, I believe, on the part of those who brought the subject forward to procure a large number of signatures, or in any way to obtain a direct representation of the Scientific bodies: the Memorial was a preliminary step, and in that stage nothing more was required, than to show that it was supported by a large number of persons of that class, from which science, if called upon, would have selected her representatives. The Memorial was presented by a deputation, and care was taken to explain to Government, that it did not emanate officially from this Society or from any other.

A detailed explanation was also entered into, showing in what way the cultivation of science would be promoted, by providing suitable accommodation for the scientific societies. Carleton ride was suggested as a convenient situation, and other sites were named in the same neighbourhood.

Reference was made to the suggested site at Kensington, and it was explained that it would be inconvenient to a large proportion of the working men of the Societies, many of them engaged in business, and whose time was most valuable, that they would be unable to attend regularly, and that the usefulness of the Societies would be thereby greatly impaired. It was also intimated that the purchase of a large tract of land at Kensington did not materially affect the question; as a small space would be sufficient for all the Societies, and therefore that a free grant at Kensington could be but little object. Should the Government accede to the Memorial, I presume that then the Societies will be regularly consulted. The site will then be pointed out, the plan of the building laid before you, and you will form your own opinion; you will decide whether to remain as you are, or to accept the enlarged accommodation, which the far-sighted liberality of the nation will have provided for you.

The interests of science appear to me to be deeply involved in the question of providing a suitable building for the Scientific Societies. It is a practical question, and we must look at it in all its bearings, as men of the world. Had there been no Societies, science would not therefore have stood still; but its progress would have been much slower. The desire of discovering the truth, which is so strongly implanted in every educated mind, would have been to some a sufficient motive for exertion; but the aid and inspiriting influence of association would have been wanting, and many active members of our scientific bodies would probably never have taken up science as a pursuit at all. The love of ease, the fascinations of society, the little prospect of advancing their material interests, through the path of scientific discovery, would have been to them a sufficient excuse for indolence. This point was very well put at the last Anniversary, by one of the distinguished men to whom you awarded a medal; he said, had he been like Robinson Crusoe on a desert island; the desire of discovering the truth would have been an inducement to work, but to obtain the good opinion of his fellow-men, associated together, was with him a very strong additional inducement. If a man, naturally gifted, and well-educated, attends scientific meetings, he will feel himself constrained to work, and therefore it is so important for the advancement of knowledge, that able men should be induced to join and attend the different societies; but nothing I think would have greater attractions than a building in a convenient central situation, where the business of science would be transacted, where there would be access to the best libraries, and where that kind of society most valued by scientific men would always be within reach.

Where the question is, shall a great country like England provide a suitable place of meeting for its scientific bodies, I should hope there are not many who would be disposed to count the cost, I scarcely think the question, *cui bono*, could be asked; should any one however ask it, I should answer, the object is to promote the increase of human knowledge, to extend the domain of reason; and "it is the understanding that sets man above the rest of sensible beings, and gives him all the advantage and dominion which he has over them." To take lower

ground, science is a part of England's greatness: without science England would be nothing, and in the race with the world, if she is forsaken by science, she will fall far behind. The exact sciences are the foundation of navigation; they are the foundation of engineering; and many of our manufactures are based on principles, which have been evolved in the researches of a refined chemistry. Besides, have we not sometimes been at fault where science was not ready to guide us? We have heard of failures in ship-building: some ships sail slowly, and are therefore of little use; others roll, and strain, and are also condemned; where we have succeeded best we have taken our models from abroad, where high science has long been urged, to lend its aid in naval construction: where such men as the two Bernouillis, Euler, Chapman and others, have been induced to examine and discuss the questions of most interest to the practical builder. In civil engineering too, our most eminent men, I am sure, would not hesitate to acknowledge their obligations to our neighbours. We have long been almost unrivalled in the applications of steam; and yet, who is there who has not sought for the theory of the locomotive in the essay of De Pampour, or the principles of steam in general in the researches of Regnault?

In military engineering too, as well as in naval architecture, has not science been sometimes on the side of our adversaries? and have we not in consequence suffered severely? But let me not be mistaken. I do not contend that science can in a moment increase our success in the arts, upon which the greatness of this country depends. If we were to say to the mathematician, give us the best lines for a ship suited to a given purpose, however profound his mathematical knowledge might be, he would fail; practice must be combined, but in due subordination with theory. It is where in a nation science is cultivated profoundly by a large class of persons, and circumstances exist tending to direct it to practice, that some men will always be found, gifted with the faculty of applying it in whatever way the interests of the country may require.

Popular science, however, will not do: it has its uses, subordinate as they are; it must be science of a high order; science as taught at our universities: there a power is created capable of effecting great objects, but in too many cases it is not applied at all, and it soon passes away without useful result. Were it possible to enlist that gigantic power into the service of the country by making our scientific Associations more inviting, by placing science in this metropolis in a position more attractive, a result would be obtained which the merest utilitarian would consider of immense value.

I deeply regret that the last accounts have brought no intelligence of Sir J. Franklin's expedition, and that science has sustained a heavy loss in the death of a distinguished French officer; the latest sacrifice to the perils of Arctic discovery.

Many now present were acquainted with Lieut. Bellot, and I am sure they will bear me out in this, that there was a singleness of purpose, and a propriety of feeling in everything he said and did, in perfect keeping with the tenor of his previous life; and though

he was here so short a time, he was regarded as a friend by all who knew him.

You have heard no doubt with sincere pride, that British valour, and British perseverance, have at length solved the problem of the north-west passage. A question of great geographical interest has thus been settled, and an important fact has been added to the data of terrestrial physics. In all future inquiries relative to the oceanic currents, the tides, the variations of temperature, the winds, and meteorological phenomena generally, it will no longer be a doubtful assumption, that the sea flows freely around the northern coast of America.

It now only remains for me to state, and I have the greatest pleasure in doing so, that your Society is prosperous. The publication of your Transactions proceeds regularly, and they continue to be, as they have been for two centuries, the records of every important addition to British science. There is no indication of a diminished anxiety to share in your labours, the candidates for admission are numerous, and your Council have had no difficulty, though acting under a heavy responsibility, in pointing out to you the required number of persons in every way worthy of the Fellowship.

So far the present system of election appears to me to have worked extremely well. The Fellowship is sought for as a high honour, and here, as at the Universities, the claims of the respective candidates are tested by responsible persons.

In this, the Royal Society differs from every other Society; and I think upon the preservation of that distinction, the welfare, the position, perhaps the existence of our Society depends. For 150 years the Royal Society stood alone; unaided it bore the whole labour of wielding the power of Association, in the cause of progressing science. Recently other Societies were formed to meet more fully the wants of individual sciences; not as rivals to the Royal Society, (in all of them our Fellows have held very prominent places) but as the most friendly allies; not dependent on the Royal Society, but fully admitting its pre-eminence.

These Societies have rendered important services; much has been effected through their means which otherwise would not have been attempted. Science has been carried out by them in the utmost detail. Besides, it is a law of human nature, that we usually form a high estimate of the importance of the pursuit we are engaged in; and in a Society limited to one science, that feeling will necessarily predominate, and will act as a stimulus to exertion. Under its influence, labour will be cheerfully borne, from which under other circumstances we should recoil with disgust. That feeling, however, would proceed too far were there not here a power to restrain it: you hold that power: you exercise a presiding influence over all the Societies. The leading members of the scientific bodies have their places here, and science is fully represented.

You look at science as a whole, and you weigh the value of every new discovery as adding to the mass of human knowledge. The honour of contributing to your Transactions is eagerly sought for,

the medals you award are in high estimation, and science is stimulated to its grandest efforts, because you are viewed by all as just and able judges. To hold securely that proud position, learning must be your distinguishing attribute; in the altered state of things it is learning which fits you for your new duties, and so long as the Fellowship is regarded as the reward of services in the cause of science of no common order, or of proved scientific eminence at the universities, so long, I think, we may predict with confidence that the Royal Society will flourish.

COLONEL SABINE,

I am happy to have the honour of handing to you the Copley Medal in charge for Professor Dove.

Three important branches of Terrestrial Physics have recently been greatly advanced, and by similar means; the adequate discussion of accumulated results of observation—I allude to the tides, terrestrial magnetism, and the distribution of heat over the surface of the globe. Our knowledge of the tides was limited, and fragmentary, till it became extended and systematized by the researches of Dr. Whewell, and notwithstanding the numerous detached observations which had long been made on the effects of terrestrial magnetism, we were still ignorant of much of the real laws of the phenomena, till under the leading influence of Gauss, not only individuals, but Governments were induced to unite in measures for obtaining the accurate and systematic observations, which in the hands of Colonel Sabine are leading to such determinate and interesting results. The labours of Dr. Whewell and Colonel Sabine have received their well-merited acknowledgements in the Medals which have been respectively awarded to them; and this year the Copley Medal has been awarded to Professor Dove for his laborious and valuable researches on the *distribution of heat over the surface of the globe*. The results obtained in all these cases constitute unquestionably great steps in the progress of science; and though labours such as these may not necessarily demand that acute discrimination and inventive faculty required for the discovery of truths before unthought-of, they do require what is scarcely less valuable, those enlarged and comprehensive views by which we are enabled to recognize real order in the midst of apparent confusion, and deduce from the most complex and intricate phenomena, the simple laws to which they may be referable. The work which Professor Dove has recently completed comprises the results obtained by him during many years, by the careful and elaborate discussion of an immense number of recorded observations of the temperature at numerous stations, and in almost every accessible region of the globe. The details of these discussions have been published by him at different times, principally in the Transactions of the Berlin Academy. In this recent work a large map exhibits the isothermal lines for January and July, the months of extreme winter and summer temperatures; while twelve small maps engraved on two sheets exhibit the isothermals for each successive month, and enable us to trace at

once their changing positions with the changing seasons. Twelve other small maps of subnormal lines show, in like manner, for each month, the difference between the actual mean temperature for that month, and the *normal* temperature (that due to the latitude) at all places situated along each abnormal line. These, with other subsidiary maps, present to us at once, and with the greatest perspicuity, the principal results of these laborious investigations.

These maps are accompanied by a considerable quantity of letterpress, containing not only descriptive details, but also a very able discussion of some of the principal results obtained, and of the general conclusions deducible from them; more especially as regards the causes of the abnormal temperatures which characterise the northern hemisphere. The influence of oceanic currents, on the temperature of the regions in which they prevail, was very inadequately appreciated before the publication of these researches. Of these currents, the most important, and infinitely the most interesting to ourselves, is that so well known as the Gulf-stream. Its immense influence in moderating the winter cold along the shores of western Europe is shown by the singularly abnormal position of the winter isothermals in that region; and not only is this fact of great interest in itself, and of first-rate importance in meteorology, but it has also enabled the geologist to form a far more accurate estimate than otherwise it would have been possible to have done, of the probable climatal influences of particular configurations of land and sea, and thus to overcome, not by arbitrary hypothesis, but by logical deduction, some of the greatest apparent anomalies in speculative geology. The former existence of glaciers in our own islands need no longer be regarded as a mystery, for it is now demonstrable that they would be the highly probable, if not the absolutely necessary consequences of any configuration of land and sea which should divert the Gulf-stream from its present course; and the geologist has no difficulty in conceiving such a configuration, not merely as a possible, but as one which probably did exist during the glacial period. I mention this as an instance of the diffusive influence of a great step in one science, on the progress of other sciences more or less directly associated with it. A further and very important conclusion has been deduced by Professor Dove from the monthly isothermals, I mean the fact that the mean temperature of the surface of the globe, as a whole, is higher when the sun is in the northern, than in the southern signs. The explanation is, that the northern hemisphere has more land than sea at the surface, and the southern much more sea than land, and that from the different action of the sun's rays on the solid and fluid surfaces, it follows that the hot summer of the northern hemisphere added to the mild winter of the southern, gives a mean of general temperature several degrees of Fahrenheit higher than the cool summer of the southern, together with the cold winter of the northern hemisphere.

It will, I doubt not, be generally felt by all who are acquainted with these researches, that Professor Dove has well merited the honour which the Council has conferred upon him in this award of the Copley Medal.

Mr. Darwin,

I have much pleasure in announcing that a Royal Medal has been awarded to you.

Adopting the views of Sir Charles Lyell, who has sought to explain natural phenomena by an appeal to the evidence afforded by still active causes, you have observed with great care, and no one has been more judicious, or more successful, in collecting facts. The frequent references made to your labours by writers on general geology, are evidence of the estimation in which they are held. Your work on Coral Reefs is a fine specimen of an able argument on facts. In that work you have brought together all the information collected by others, as well as by yourself, and you have explained the facts observed relating to the distribution of coral reefs, the conditions favourable to their increase, the rate of their growth, and the depth at which they are found; and thus laying a sound foundation to reason upon, you have shown that, with few exceptions, the old theory, that these reefs have been formed on the edges of submarine craters, and generally that the rocky or other basis on which the corals have grown had been *elevated, is incorrect*; and, on the contrary, that the true theory is, that the surface has been gradually brought down to the proper level, a depth for the growth of corals, by gradual subsidence. No one probably can read your book without assenting to the general truth of your reasoning; and as it places the fact of *subsidence* beyond doubt, a fact more difficult to prove than elevation, and exhibits it on a scale of magnitude and generalization quite commensurate with that of elevation, I think it must be accepted as one of the most important contributions to modern geology.

In your Monograph on the Pedunculated Cirripeds, you have treated generally of the structure, economy, and zoological relations of these animals, and given a systematic arrangement and description of the different species. In the accomplishment of your task, you have not only made use of previously existing materials with sound and enlightened criticism, but, by the discovery of new facts and the promulgation of original views, contributed most materially to advance the department of knowledge to which your researches more immediately belong, and rendered valuable service to physiological science in general.

In the course of your inquiries you have confirmed and widely extended the observations of your predecessors respecting the larval condition of the Cirripeds, and have shown that all the perfect Lepads and Balanids pass through successive stages of metamorphosis. You have also added largely to our knowledge of the anatomy of the larva, and brought to light the curious fact, that in one of its stages its mouth is altogether rudimentary, and perfectly closed up by the external covering, so that the creature in this stage is in fact a "locomotive pupa," incapable of feeding. You have further observed that the prehensile antennæ with which the larva fixes itself in its final change, invariably remain permanent in the adult animal

at the attached end of its peduncle, and in many cases afford important characters for zoological discrimination.

The knowledge thus gained from the study of development is most sagaciously and happily applied by you to explain the homological relations between the Cirripeds and Crustaceans; and in this way you have conclusively shown that the peduncle of the mature Lepad corresponds with the three anterior segments of the Crustacean. Again, by your discovery of *Proteolepas*, a new parasitic Cirriped of low organization, you have been able successfully to compare the remaining segments of the body in the two classes; for whilst the chain of evidence is in some measure broken by the absence of two segments near the middle of the series in Cirripeds generally, the missing links are supplied by the newly-discovered animal referred to.

The existence of an eye with a pair of ophthalmic ganglia in adult Lepads, as had been previously shown in Balanids,—the presence too of organs seemingly intended for hearing and smelling—the chemical nature of the tegumentary coverings—the cement-gland and ducts, yielding a plastic material for attaching the peduncle and for other special purposes in particular instances, and the singular organic connexion between that gland and the ovaries, are all most interesting discoveries in comparative anatomy, first made known in your work.

Some very singular facts respecting the reproductive function in the Lepadidæ have been brought to light through your researches. You observed that the ova, on leaving the ovary, are gathered in a layer underneath the internal lining of the sac or mantle, from whence they are freed and extruded by the process of moulting, and then form the ovigerous lamellæ already known. Again, you have clearly established, that, contrary to the hitherto received opinion, there are species of Lepadidæ consisting of individuals of distinct sexes, the male being parasitic on the female; and in certain other species you have discovered a condition hitherto unknown in the animal kingdom, namely, bisexual individuals impregnated by parasites simply of the male sex, which you name "complemental males,"—a fact unquestionably of first-rate interest in relation to the physiology of the reproductive function in general.

Besides these more important observations, there are many more of lesser mark to be met with in the descriptions of particular species,—descriptions, it may be observed, which are founded on a careful examination, not only of the external characters, but of the internal structure of the animals in question, in specimens obtained from every available source, both at home and abroad; and the zoological distinctions are rendered more precise and intelligible than heretofore, by the introduction of a consistent and philosophical nomenclature.

The President then called upon Mr. Christie to read the following obituary notices of some of the deceased Members.

Jonathan Pereira, M.D., was born on the 22nd of May, 1804, in the parish of Shoreditch, London. At ten years of age he was placed in a Classical Academy in Queen Street, Finsbury, where he remained about four years, and distinguished himself as one of the most promising pupils in the school.

Dr. Pereira's education was now directed to the pursuit of surgery, but his appointment to the office of Resident Medical Officer of the General Dispensary in Aldersgate Street in 1823, led to his devoting himself more especially to the practice of medicine.

In 1826 he succeeded Dr. Clutterbuck as lecturer on Chemistry at the Aldersgate Street Dispensary, His first lecture was devoted to an account of the rise and progress of Chemistry from the earliest date to which the history of the science could be traced, and comprised a notice of the latest discoveries. The theatre was crowded to excess, and the lecture created no little sensation from the profusion of illustrations, the amount of information, and the style of his delivery. Among other illustrations he exhibited bromine, which had recently been discovered by Balard, of Montpellier.

His cultivation of chemistry at that time in connexion with medicine, naturally directed his attention to the subject of the substances used as medicinal agents, and in 1824 we find him publishing a translation of the 'London Pharmacopœia.' This was followed by 'A Manual for the Use of Students,' 'A general Table of Atomic Numbers, with an Introduction to the Atomic Theory,' and other works for the use of those who were pursuing their medical studies with a view to passing the usual examinations. He afterwards published numerous papers on the adulteration and properties of drugs; and thus prepared himself for his great work—that on which his reputation as a physician and man of science will principally rest,—his 'Elements of Materia Medica.' The outlines of this work first appeared as a course of lectures in the London Medical Gazette. This work contained by far the most complete and accurate account of substances used in medicine that had ever been published. Not only were sources of medicine and their commercial history fully treated therein, but the author entered with great caution and skill into inquiries connected with the action of remedies: and thus his book became at once a standard of reference for all who were engaged in the business of selling drugs and chemicals, or in the duty of prescribing them as medicines. The first volume and a part of the second of a third edition of this work had been published at the time of the author's decease. In 1843, Dr. Pereira published a treatise on diet; which at the time of its appearance was one of the most philosophical works that had yet been produced on the subject of the food of man, and 'Lectures on Polarized Light,' the best familiar exposition of that abstruse subject in our language. He also contributed numerous articles to societies, journals, reviews, etc.

His death has left a great void in English Pharmaceutics, as no one in England had a greater amount of knowledge on that subject. As a lecturer, he secured the attention of his class by an earnestness of purpose, aptness of experimental illustration, and the practical bearing of his remarks.

Early in the year 1832 he resigned his office at the General Dispensary, and on this occasion a silver salver was presented to him by the Governors of that Institution, as a memento of their regard and esteem. In the following September he married, and established himself in general practice in Aldersgate Street.

In the winter of 1832 he became Professor of Materia Medica in the New Medical School in Aldersgate Street; and at the same period succeeded Dr. Gordon as Lecturer on Chemistry at the London Hospital.

His connexion with the London Hospital procured him the opportunity of being appointed Physician to that extensive institution. In 1841 he procured the licence to practise in London from the College of Physicians. He was elected a Fellow of that body in 1845. On the establishment of the London University, he was appointed Examiner in Materia Medica and Pharmacy,—a position which he held till his death. He took great interest in the establishment of the Pharmaceutical Society, and delivered lectures on Materia Medica in connexion with that body.

Dr. Pereira was the distinguished and highest representative of that science which involves the history, properties, and uses of drugs, better known on the Continent than in England by its name of Pharmacology. At the recent meeting of the Association for the Advancement of Science at Wiesbaden, there was a Pharmacological Section specially devoted to the subject, attended by upwards of two hundred members. 'The Elements of Materia Medica and Therapeutics,' to which Dr. Pereira gave the best years of his life, is considered to be one of the most elaborate and thoroughly worked-out productions of modern science. Compared with similar pharmaceutical works of France and Germany, it is remarkable for its diversity of character, excelling greatly in the amount of commercial and other practical information, in the preciseness and value of its notes, and in the scrupulous exactness with which its statements are authenticated by references to their original source. Where obscurity attached to any statement, the author was careful to express it with a mark of doubt. As a painstaking and conscientious writer Dr. Pereira was unsurpassed, and his work contains nothing that may not be thoroughly relied on for its accuracy. He also added largely to the botany of pharmacy.

Dr. Pereira had the happiness of enjoying during his life, large honours, and there is little doubt, had he been spared, he would have reaped the more substantial rewards of a lucrative practice.

He was elected a Fellow of the Royal Society in 1838, and was chosen on the Council in 1842–44. He was also a Fellow of the Linnean and other Scientific Societies, and was in constant communication with the learned of all countries.

His death, which took place on the 20th of January last, was the result of an accident. Being engaged at the College of Surgeons on some scientific business, he fell down a flight of steps, which led to the rupture of an artery, and terminated his life at the early age of forty-nine, not however without leaving behind a name which will occupy a prominent place in the history of the science of the nineteenth century.

Hugh Edwin Strickland, Esq., was descended from an ancient Baronet family in Yorkshire, one of whose ancestors was Lord Strickland in the Protectorate of Cromwell. From early life Mr. Strickland was a zealous student of Natural History, and in consequence of his proficiency in all its branches, but particularly in Ornithology and Geology, was appointed to succeed Dr. Buckland as Reader in Geology at the University of Oxford, and was subsequently elected President of the Ashmolean Museum. Besides frequent and valuable zoological, botanical, and geological contributions to periodical publications, Mr. Strickland obtained great celebrity by the publication of an elaborate volume on the Extinct Dodo, which he wrote in conjunction with Dr. Melville. Mr. Strickland was elected a Fellow of the Royal Society in 1852.

His death was most melancholy. He left Hull, where he had been attending the Meeting of the British Association in September last, to visit an interesting geological locality, on the line of railway between Retford and Gainsborough. Here, while engaged in taking a sketch, and imprudently standing on the railway, he was run down by an express train, and in an instant was a mangled corpse.

François Arago was born on February 26th, 1786, at Estagal near Perpignan, in the department of the Oriental Pyrenees. His father had only a moderate patrimony, and was Treasurer of the Mint at Perpignan. Being intended for the legal profession, or some public office, the early education he received was entirely literary, but having a predilection to become an officer in the Artillery, he entered himself at the Polytechnic School at Paris, and with but little aid from masters, instructed himself from the works of Euler, Lagrange, and Laplace, instead of resorting to the manuals where science is retailed at second-hand. In 1803, when he was eighteen years of age, he was received by the younger Monge at Toulouse, and a year afterwards, on account of the superiority of his scientific attainments to the rest of his companions, he was recommended by the elder Monge to the Observatory at Paris. This occasioned a change in his original destination, and opened for him a career far more useful to science and mankind. Whilst in the capacity of observer at this national establishment, he occupied himself with researches of the greatest value to astronomy and physics.

The death of the astronomer Mechain had interrupted the measurement of the meridian of France, undertaken to determine the figure of the earth, and to establish, on a scientific basis, the unit of the decimal system of measures adopted by the National Convention.

For this object, Delambre and Mechain had already measured the part of the meridian between Dunkirk and Barcelona. To continue this operation to the Balearic islands, Messrs. Biot and Arago set out for Spain in the year 1806, and were joined by the Spanish commissioners, Chaix and Rodriguez. In April, 1807, M. Biot returned to Paris to announce the results which had already been obtained, and Arago was left with M. Rodriguez to perform all the operations necessary to unite, geodesically, the islands of Majorca, Ivica, and Formentera; he thus measured, by means of a single triangle, a meridional arc of a degree and a half. At this time, rumours of the approaching war between France and Spain caused the Majorcans to regard with suspicion the nocturnal signals made on the heights of Galatzo, which they imagined were intended to direct the march of the French troops; Arago was thereupon taken up as a spy, and imprisoned, on the 2nd of June, 1808, in the citadel of Belver, from whence he contrived to escape with his instruments, and embarked on the 28th of July for Algiers.

The French consul procured him a passage, on the 13th of the same month, for Marseilles; but on entering the bay of Lyons, when in sight of the coast of Provence, the ship he was on board was captured by a Spanish privateer, and carried to Rosas on the 16th of August. After residing for some time in a windmill, and passing himself off as a travelling pedler, he was imprisoned, with the companions of his voyage, on the 25th of September, in the fort of the Trinity, and on the 17th of October was thrown into the hulks of Palamos, where he underwent great hardships, and was often left for two days together without food. It happened that the Algerine vessel in which Arago was captured, contained two lions sent as a present by the Dey to the Emperor Napoleon; one of these animals had died in consequence of neglect or ill-treatment on the part of the Spaniards, and the Dey threatened reprisals on the Spanish government unless satisfaction was given, and the vessel immediately restored. On the 28th of November, 1808, Arago reimbarked for Marseilles, but in consequence of the incompetence of the pilot, after being driven about the Mediterranean for several days, was landed at Bougie, from whence he proceeded on foot, disguised as an Arab, to Algiers. He arrived at this city on the 26th of December, but a revolution had taken place immediately before; the former Dey had just been beheaded, and disputes having arisen between the French and Algerine governments, the new Dey refused Arago permission to depart, and, had it not been for the protection of the Danish consul, the future academician would have been sent as a slave to the galleys. Fortunately, another revolution broke out; the new Dey was hung, and Arago was enabled to leave Algiers on the 21st of June, 1809; but his misfortunes had not yet entirely ceased, for when he was in sight of Marseilles, the vessel in which he had embarked was pursued by an English frigate, from which however it escaped, and Arago was safely landed, after his three years of adventure, with all his instruments, at Marseilles on the 2nd of July.

On the 17th of September, 1809, Arago was nominated to succeed Lalande as member of the Academy of Sciences by forty-seven out of fifty votes. He was then only twenty-three years of age.

A short time after Arago's admission into the Institute, he was appointed professor at the Polytechnic School, where, on account of the variety and extent of his knowledge, he was able to give successively five courses on different subjects. He was also chosen Examiner of the Engineer and Artillery officers leaving the School of Application at Metz. In 1812 the Bureau of Longitudes appointed Arago to give a course of lectures on astronomy at the Observatory, and this course was continued until 1845; a more eloquent expounder of the truths of science was never heard within these or any other walls, and crowded audiences from every class of society followed, with profit and pleasure, the facile expositor of subjects so difficult. It was justly said by the present ruler of France, when in his captivity at Ham, that Arago "possessed, in a high degree, those two faculties so difficult to meet in the same man; that of being the grand priest of science, and of being able to initiate the vulgar into its mysteries."

In 1821, Arago was engaged in making geodesical observations on the coasts of France and England, and in 1822 he was nominated member of the Bureau of Longitudes. The notices, which from 1824 to the present time, Arago furnished to the Annuaire of the Bureau of Longitudes, for the instruction of the unlearned public on various important points of science, are perfect models of what such writings should be. In the words of M. Combes, President of the Academy, "They will always be reperused with the same pleasure by men of science and by the ordinary reader; in them we find an admirable clearness, with an erudition as correct as it is extensive, and joined thereto, the most rigorous accuracy in the statement of the phenomena, and the consequences which result from them."

On the 7th of June, 1830, Arago obtained the honour which he valued the highest, being elected to succeed Fourier as Perpetual Secretary of the Academy of Sciences. He received on this occasion thirty-nine out of forty-four votes.

After the revolution of 1830 broke out, Arago entered into political life; he was elected deputy for his native department, and became a member of the Municipal Council of Paris. In both these capacities he had many opportunities of rendering service to science and advancing public improvements.

The revolution of 1848 evolved another change in the fortunes of Arago; he became one of the members of the Provisional Government, and was nominated Minister both of the Marine and of War. On his retirement from these onerous offices, for his services in which he never made the slightest demand upon the national treasury, disheartened with the state of public affairs, he entirely relinquished politics, and devoted himself with renewed ardour to his duties as Secretary of the Academy; he commenced putting in order and perfecting his numerous unpublished researches, and continued to do so until increasing infirmities prevented him. In May 1852

he was called upon, with all other public functionaries, to take the oath of allegiance to Louis Napoleon, as President of the Republic; this his antecedents and his convictions would not allow him to do, and he resolved, rather than put this constraint on his conscience, to abandon the establishment with which his name had been so gloriously identified. Happily, he was spared this wrong; the Prince President authorized his Minister to inform him, that he made "a special exception in favour of a philosopher whose labours had rendered France illustrious, and whose existence the Government would be loath to sadden." Arago did not long survive this event; a complication of disorders was carrying him to the grave; he returned, after a painful journey to his native place, seriously indisposed, and died at the Observatory, on the 2nd of October in the present year. His funeral took place on the 5th, at the cemetery of Père la Chaise; a brigade of infantry marched with the procession, and the Emperor was represented at the ceremony by Marshal Vaillant, Grand Marshal of the palace. Though it rained incessantly, above 3000 persons attended the funeral cortège.

Arago was but once married, and had the misfortune to lose his wife, a lady of great accomplishments, in 1829. He has left two sons: Emanuel, a member of the Parisian bar, and formerly representative with his father for the Oriental Pyrenees; and Alfred, a painter of distinguished reputation.

It now remains to give a short retrospect of the most prominent original investigations in physical science made by this indefatigable philosopher during the intervals of a busy and sometimes agitated life.

The important discoveries of Malus relating to polarized light attracted the attention of Arago strongly to this new and fertile field of physical research, and on the 11th of August, 1811, he read to the Academy a memoir abounding with new and beautiful facts, each forming a starting-point for subsequent investigations which have extended in no mean degree our knowledge of the laws of light. In this valuable communication, M. Arago examined, for the first time, the changes in the properties of polarized rays when they pass through plates of mica, sulphate of lime, and other crystals; he showed that they acquire the property of being divided by a bi-refringent prism into two complementary coloured pencils; and he examined the changes of colour and intensity dependent on the thickness of the plate, its inclination to the ray, its rotation in its own plane, and the rotation of the analysing prism; he also ascertained that when the emergent light was reflected from a glass plate at the polarizing angle, only a single-coloured image appeared, which changed to the complementary colour when the mirror was turned round 90°, the angles of incidence and reflexion remaining constant. He also discovered the different remarkable modifications which a polarized ray undergoes when transmitted through a thick plate of rock crystal cut perpendicular to its axis, viz. that it is analysed by a bi-refringent prism into two complementary rays which remain the same, however the plate is turned in its own plane, and pass through all changes of colour in successive order, when the analysing

prism is turned round; phænomena very different from those of the crystalline plates he at first examined. These experiments, followed by the more extensive researches of Biot, Fresnel, Brewster, and many more recent investigators, have created a beautiful and important branch of physical optics, designated that of chromatic polarization. In this memoir it was also first announced, that while the light from a clouded sky undergoes no modification, that reflected from the atmosphere when the sky is unclouded is polarized, the intensity of the polarization varying with the hour of the day, and the position of the point with respect to the sun.

In 1816 Arago published a fact which has been generally received as an *experimentum crucis* between the two rival theories of light, that of emission and that of undulations. Dr. Young had shown that two rays of light, emanating from the same source, act conjointly when they pass over equal paths, or paths differing by an even number of times a certain very small quantity, and that they destroy each other when this difference is an uneven number of times the same quantity; whence it follows that the interference of two rays under the stated circumstances produces a series of fringes alternately dark and bright. Dr. Young further ascertained that the interposition of an opaque screen in the path of one of the rays completely prevents the formation of the fringes. Arago ascertained that if one of the rays be made to pass through a thin transparent film, such as glass, the fringes are displaced towards the side to which it is applied, the magnitude of the displacement depending on the thickness of the film. This proves that the transmitted ray has been retarded in its passage, a result conformable with the theory of undulations, but in direct opposition to that of emission. Arago subsequently showed how this principle might be employed to measure the minutest differences in the refractive densities of bodies, to resolve various delicate questions in physics, and in the construction of new meteorological instruments.

In 1819 Arago and Fresnel published their joint experiments on the action which rays of polarized light exert on each other. By a variety of ingenious methods, they proved that rays polarized in the same plane mutually interfere with each other, producing fringes as in the case of ordinary light; while rays polarized in planes at right angles manifest no appreciable action on each other. They also showed that two rays primitively polarized at right angles to each other and afterwards brought to similar planes of polarization, produce fringes only when they have proceeded from a pencil originally polarized in the same plane. These new properties enabled Fresnel to give a *complete* explanation of the production of colours in crystalline plates, which Dr. Young had *before* referred to the interference of the transmitted rays, though he was unable to explain in what circumstances the interference took place, or why we see no colours unless *polarized* light be transmitted through the crystalline plates.

Hitherto Arago's original researches had been confined to subjects connected with Astronomy and Physical Optics, but the great discovery of Œrsted gave his inventive ingenuity another direction.

On the 4th September, 1820, he gave an account to the Academy of the recent investigations of the Danish philosopher, and was in consequence charged to repeat them before that body. In preparing these experiments he ascertained some hitherto unobserved phenomena. M. Œrsted had discovered the action which the voltaic current exerts on a steel needle *previously magnetized*; M. Arago found that the same current developes the magnetic power in bars of iron or steel, which are at first deprived of this property, and that the magnetism thus communicated to soft iron is temporary, ceasing with the removal of the current, while that imparted to steel is permanent. He also found, adopting a suggestion of Ampère's, that steel needles were more strongly magnetized by placing them within a helix forming part of the circuit. These discoveries subsequently led, in the hands of others, to the invention of the electro-magnet and its valuable applications; also to a ready means of making permanent magnets of great power.

On the 22nd of November, 1824, Arago communicated to the Academy the results of a series of experiments, showing the influence which metals, not magnetic, and other substances exert on the magnetic needle; this effect being to diminish rapidly the amplitude of the oscillations without sensibly changing their duration: and on the following 7th of March he announced a still more remarkable discovery, the reverse, as it were, of the preceding. Since a needle in motion is stopped by a plate at rest, M. Arago thought that a needle at rest ought to be carried along by a plate in motion; and he accordingly found thaton rotating a copper plate, for instance, beneath a magnetic needle, the needle was moved out of the magnetic meridian and stopped in a position more distant from it in proportion as the rotation of the plate was more rapid; and if this motion were sufficiently quick, the needle rotated itself continuously round the thread from which it was suspended. He added some further facts relating to this subject on July 23rd, 1826. These beautiful experiments proved the important fact, that bodies, which in a state of rest exert no action on a magnet, become capable, when they are in motion, of acting upon it as if they are magnetic. Mechanical motion was thus, for the first time, shown to play an important part in the manifestations of electric phenomena. No satisfactory explanation was, however, given of these experiments until Faraday, in 1832, referred them to the more general laws of electro-magnetic induction which he had then discovered.

In 1839, Arago laid before the Academy a proposed system of experiments by means of which the theory of emission and that of undulations might be submitted to decisive proofs. The object of this communication was to show that the method invented and employed by Mr. Wheatstone to measure the velocity of electricity in metallic conductors, was equally applicable to measure the comparative velocities of light in air and in a liquid. By following the indications of this memoir and employing Mr. Wheatstone's revolving mirror, aided by many original and ingenious arrangements of his own, M. Foucault has recently completely succeeded in realizing these anticipations of Arago.

A passing allusion is all that can be made to other important in-

vestigations of this active-minded philosopher. We owe to him the discovery of a neutral point in the polarization of the atmosphere; the determination, by simultaneous hourly observations with M. Kupffer of Kasan, of the synchronous perturbations of the magnetic needle at places very distant from each other; a series of delicate experiments in photometry, and various experimental verifications of Fresnel's theory; a new method of measuring the diameters of the planets; and many valuable astronomical and meteorological observations.

In the autumn of 1834 Arago visited this country. His principal object was to make himself acquainted with the methods of observation employed at the Royal Observatory at Greenwich, then under the direction of Mr. Pond. He also attended the meeting of the British Association which was that year held at Edinburgh; on this occasion he received a diploma of the freedom of the city, and was nominated an honorary member of the Association. He made several interesting communications to the Physical Section; and the Committee having requested him to state his views as to any points on which it appeared to him it might be useful for the British Association to co-operate with the Institute of France, he noticed in particular "the great advantage which might be expected to accrue to magnetical science from the establishment of observatories furnished with adequate instruments, and under the superintendence of a competent observer, throughout the extensive possessions of the British empire, and dwelt on the necessity of arranging magnetical observations upon a uniform and well-approved plan."

The ensuing year the Committee of the British Association acted on this suggestion, by resolving that an application should be made to the Government to carry it into effect. In 1836, the Royal Society, on whose attention the subject was strongly urged by Baron von Humboldt, added its weight to the recommendation, and the joint endeavours of these two bodies resulted in the establishment by Her Majesty's Government and the East India Company, of numerous magnetic observatories in widely separated parts of the British dominions, co-operating with others instituted in various parts of Europe and the United States of America. It is to be regretted, that, notwithstanding M. Arago's strong interest in this great scientific combination, no corresponding observations were made in Paris; Algiers having been the only station established at his suggestion in the French Empire.

M. Arago was elected a foreign member of the Royal Society in 1818, and on two subsequent occasions the Council of that body marked their sense of his high scientific merits by adjudging to him in 1825 the Copley Medal, "for his discovery of the magnetic properties of bodies not containing iron," and again in 1850, the Rumford Medal, "for his experimental investigations on polarized light."

The late Rev. Charles Turnor, M.A., F.R.S., and F.R.A.S., was an ardent benefactor to Science. He was born in Lincolnshire on the 10th of August, 1768; and after receiving the first rudiments of education at Grantham, graduated in Trinity College, Cambridge. On the 15th of May, 1802, he was instituted to the vicarage of

Wendover, at the foot of the range of the Chiltern Hills, in Buckinghamshire; the duties of which he faithfully exercised for many years. In 1818 he was collated to the prebendal stall of Sutton-in-the-Marsh, in the cathedral church of Lincoln, by Bishop Pretyman Tomline, on the death of the Rev. Dr. Charles Burney; and in 1825, obtained a dispensation to hold the vicarage of Milton Ernest, in the county of Bedford. He resigned this vicarage, and all other preferments, excepting his prebend, in 1837. Beloved and respected by all who had the pleasure of his acquaintance, he passed a long life in active utility, and died on the 12th of January, 1853.

Mr. Turnor was descended from an ancient family of Stoke Rochford, and Penton House in the county of Lincoln. Among his ancestors were Sir Edmund Turnor, Paymaster to the Forces in the reign of Charles I., who was taken prisoner at the battle of Worcester in 1651; and his brother Christopher Turnor, one of the Barons of Exchequer in 1660. The property of Sir Isaac Newton, being only three miles from Stoke Rochford, was purchased by the family about four years after Newton's death, and its integrity remains much the same as in Sir Isaac's time. This circumstance led Mr. Charles Turnor during a series of years to collect medals, papers, portraits, and all he could find connected with the illustrious philosopher, regardless of trouble or expense; and in 1847 he erected an obelisk, 64 feet high, in the park of Stoke Rochford, with an appropriate inscription written by himself.

The interest which Mr. Turnor felt in the progress of science, is amply evinced by his valuable and useful donations to various institutions, and the liberal sums he bequeathed them. Besides being a real patron of the sterner branches of knowledge, he was an excellent artist, and a critical judge of all objects of taste.

There remains but to add, that Mr. Turnor bequeathed the above-mentioned interesting memorials of Newton to the Royal Society, on the understanding that the collection, which he did not live to finish, should be completed.

On the motion of the Rev. Baden Powell, seconded by Dr. Roget, the best thanks of the Society were given to the President for his excellent Address, and his Lordship was requested to permit the same to be printed.

The Statutes relating to the election of Officers and Council having been read, and the Rev. John Barlow, and Captain Younghusband having, with the consent of the Society, been nominated Scrutators, the votes of the Fellows present were collected.

The following Nobleman and Gentlemen were reported duly elected Officers and Council for the ensuing year:—

President—The Earl of Rosse, K.P., M.A.

Treasurer—Colonel Edward Sabine, R.A.

Secretaries— { Samuel Hunter Christie, Esq., M.A.
William Sharpey, M.D.

Foreign Secretary—Rear-Admiral W. H. Smyth.

Other Members of the Council.—Thomas Bell, Esq.; Rev. James Booth, LL.D; Warren De la Rue, Esq.; Captain Robert Fitzroy, R.N.; Thomas Graham, Esq., M.A.; William Robert Grove, Esq., M.A.; Joseph Dalton Hooker, M.D.; Thomas Henry Huxley, Esq.; Henry Bence Jones, M.D.; George Newport, Esq.; John Phillips, Esq.; Sir Frederick Pollock, M.A.; Rev. Baden Powell, M.A.; George Gabriel Stokes, Esq.; William Tite, Esq.; Charles Wheatstone, Esq.

Statement of the Receipts and Payments of the Royal Society between Dec. 1, 1852, *and Nov.* 30, 1853.

RECEIPTS.

				£	s.	d.
Balance in the hands of the Treasurer at the last Audit ..				182	0	1
Weekly Contributions, at one shilling				39	0	0
Quarterly Contributions at £4				1080	0	0
17 Admission Fees				170	0	0
6 Compositions for Annual Payments at £60				360	0	0
3 Compositions for Annual Payments at £40				120	0	0
One year's rent of estate at Mablethorpe: due at Michaelmas 1852	116	16	0			
One year's Income Tax	3	8	0			
				113	8	0
One year's rent of estate at Acton: due at Michaelmas 1852	52	9	2			
One year's Income Tax	1	9	9			
				50	19	5
One year's Fee farm rent of lands in Sussex: due at Michaelmas 1853				19	4	0
One year's rent from Royal College of Physicians				3	0	0
Dividends on Stock:—						
One year's dividend on £14,000 Reduced 3 per cent. Annuities	420	0	0			
Less Income Tax	12	5	0			
				407	15	0
One year's dividend on £7705 9*s.* 10*d.* 3 per cent. Consols	231	7	3			
Less Income Tax	6	18	11			
				224	8	4
Half a year's dividend on £1359 18*s.* 6*d.* ..	20	7	6			
Less Income Tax	0	11	4			
				19	16	2
One year's dividend on £3452 1*s.* 1*d.* 3 per cent. Consols, produce of sale of premises in Coleman Street........................	103	11	2			
Less Income Tax	3	0	4			
				100	10	10
Carried forward..........				2890	1	10

				£	s.	d.
Brought forward..........				2890	1	10
Donation Fund.						
One year's dividend on £5331 10*s.* 8*d.* Consols	159	18	6			
Less Income Tax	4	13	0			
				155	5	6
Rumford Fund.						
One year's dividend on £2430 12*s.* 5*d.* Consols	72	17	9			
Less Income Tax	2	1	9			
				70	16	0
Fairchild Fund.						
One year's dividend on £100 New South Sea Annuities				3	0	0
Bakerian Lecture and Copley Medal Fund.						
One year's dividend on £366 16*s.* 1*d.* New South Sea Annuities	10	18	0			
Less Income Tax	0	6	2			
				10	11	10
Wintringham Fund.						
One year's dividend on £1200 Consols	36	0	0			
Less Income Tax	1	1	0			
				34	19	0
Miscellaneous Receipts:—						
Sale of Philosophical Transactions, Abstracts of Papers, and Catalogues of the Royal Society's Library				291	7	11
Sale of Three Acres Two Roods and One Perch of Acton Estate to Railway Company				1067	10	0
Bequest from the Rev. C. Turnor to complete Newton Memorial				250	0	0
Balance from Balloon Committee due to Donation Fund				18	0	0
Total Receipts......				£4791	12	1

PAYMENTS.

				£	s.	d.
Fairchild Lecture.—The Rev. J. J. Ellis, for delivering the Fairchild Lecture for 1853				3	0	0
Bakerian Lecture.—Colonel Sabine, for delivering the Bakerian Lecture for 1853				4	0	0
Salaries:—	£	s.	d.			
S. H. Christie, Esq., one year, as Secretary..	105	0	0			
Thomas Bell, Esq., one year, as Secretary ..	105	0	0			
Ditto for Index to Phil. Trans.	5	5	0			
Admiral Smyth, one year, as Foreign Secretary	20	0	0			
Charles R. Weld, Esq., one year, as Assistant-Secretary	300	0	0			
Mr. White, one year, as Clerk	100	0	0			
Porter	40	0	0			
				675	5	0
Carried forward..........				682	5	0

				£	s.	d.
Brought forward..........				682	5	0
Purchase of £1359 18*s*. 6*d*. 3 per cent. Consols				1367	10	0
Fire Insurance, on the Society's Property				45	1	6
Gratuity to Bank Clerks				1	1	0
Bills :—						
Taylor :						
Printing the Phil. Trans., 1852, part 2 ..	208	9	9			
Ditto, 1853, part 1..................	79	17	6			
Ditto, 1853, part 2..................	53	10	0			
Ditto, Proceedings, Nos. 90—97; Circulars, Lists of Fellows, Ballot-lists, Statement of Payments, Minutes of Council; Government Grant Committee, Notices, &c. &c.	144	19	9			
				486	17	0
Basire :						
Engraving and Printing Plates in Transactions, 1852, part 2	121	17	6			
Ditto, 1853, parts 1, 2 and 3	208	10	6			
Aldous :						
For Engraving	6	6	0			
Gyde :				336	14	0
For Wood Engraving.........................				10	10	0
Bowles and Gardiner :						
Paper for the Phil. Trans., 1852, part 2, and 1853, parts 1 and 2				202	0	0
Gyde :						
Boarding and Sewing 800 Parts of Phil. Trans., 1852, part 2	22	18	0			
Ditto, 1853, part 1..................	11	6	2			
Ditto, part 2	9	14	2			
Ditto, Copies for Authors............	15	19	7			
				59	17	11
Tuckett :						
Bookbinding	49	17	3			
Hyde :						
For Stationery	10	9	0			
Saunderson :						
For Shipping Expenses	9	16	6			
Brecknell and Turner :						
Candles, and Lamp Oil	42	1	6			
Carried forward.........				3191	16	5

				£	s.	d.
Brought forward				3191	16	5
Meredith:						
Mats, Brushes, Fire-wood, &c.	7	7	3			
Cubitt:						
For repairs and relaying Carpets, &c.	25	19	5			
Shoolbred and Co.:						
For Furniture	9	16	6			
Laing:						
For Painting	19	19	3			
Charlton:						
For Cases and Shelves	3	19	6			
Humphries:						
For Livery	5	10	0			
Tea, Waiters, &c. at Ordinary Meetings	32	15	4			
Powers, Cleaning Rooms and Books	10	16	0			
				228	7	6
Books purchased:						
Dulau and Co.: for Books	29	0	0			
Taylor: for ditto	22	2	3			
Gould: for ditto	15	15	0			
Williams and Norgate: for ditto	7	10	0			
Quaritch: for ditto	37	14	0			
Bumstead: for ditto	16	13	0			
Second-hand, ditto	44	12	0			
				173	6	3
Taxes:						
Land and Assessed Taxes	16	5	10			
Income Tax	4	19	2			
				21	5	0
Illuminating Sheets, for Newton Memorial				26	12	0
Wintringham Fund:						
Governors of Foundling Hospital				34	19	0
Petty Charges:						
Postage and Carriage	38	10	3			
Expenses on Foreign Packets, &c.	3	1	3			
Stamps	0	12	6			
Charwoman's Wages	26	0	0			
Extra Cleaning	6	0	0			
Miscellaneous expenses	34	4	4			
				108	8	4
Balance in the hands of the Treasurer				1006	17	7
Total				£4791	12	1

EDWARD SABINE, *Treasurer*.

November 30*th*, 1853.

Estates and Property of the Royal Society.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16*s*.
Estate at Acton, Middlesex (29 A. 0 R. 1 P.)
Fee farm rent in Sussex, £19 4*s*. per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
£14,000 Reduced 3 per cent. Annuities.
£21,478 12*s*. 6*d*. Consolidated Bank Annuities.
£466 16*s*. 1*d*. New South Sea Annuities.

The Receipts during the past year, exclusive of the Balance of the last year and the Receipts from the Donation, Rumford, Fairchild, Wintringham, Bakerian and Copley Fund, Turnor Bequest, and sale of portion of Acton Estate, were £2999 9*s*. 8*d*.

The Expenditure during the same period, exclusive of £1067 10*s*. 0*d*. invested in the Funds, arising from the sale of land, and £300 received for compositions in 1852, and of sums paid on account of Trust Funds, was £2355 13*s*. 6*d*.

Excess of Income over Expenditure including Compositions £643 16*s*. 2*d*.

The augmentations to the Library included in the Expenditure of the present year, exclusive of binding, amount to £173 6*s*. 3*d*.

Cost of printing the Transactions, 1852, Part II. and 1853, Parts I. and II. £950 19*s*. 2*d*.

Balance in hand belonging to the Wintringham Fund, £34 19*s*. 0*d*.

The following table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s*. Annually.	Paying £4 Annually.	Total.
December 1, 1852..	11	49	420	15	272	767
Since elected......	......	......	+8	......	+11	+19
Since compounded	......	......	+2	......	−2	
Defaulters	......	......	......	......		
Withdrawn	......	......	......	......		
Since deceased	......	−2	−16	−1	−8	−27
November 30, 1853	11	47	414	14	273	759

Receipts by Annual Contributions.

	£	s.	d.
1830	£363	4	0
1831	286	0	0
1832	255	6	0
1833	283	7	6
1834	318	18	6
1835	346	12	6
1836	495	0	0
1837	531	0	0
1838	599	4	0
1839	666	16	0
1840	767	4	0
1841	815	12	0
1842	910	8	0
1843	933	16	0
1844	1025	16	0
1845	1010	0	0
1846	1074	0	0
1847	1116	8	0
1848	1122	16	0
1849	1130	16	0
1850	1146	4	0
1851	1117	12	0
1852	1135	0	0
1853	1119	0	0

December 8, 1853.

COLONEL EDWARD SABINE, R.A., V.P. & Treas., in the Chair.

The Chairman stated to the Meeting that the President had appointed the following gentlemen Vice-Presidents:—

Colonel Edward Sabine; Thomas Bell, Esq.; Thomas Graham, Esq.; Sir Frederick Pollock, M.A.; the Rev. Baden Powell, M.A.; Charles Wheatstone, Esq.

A paper was read, entitled "On some of the Products of the Decomposition of Nitrotoluylic Acid." By Henry M. Noad, Ph.D., Lecturer on Chemistry at St. George's Hospital. Communicated by A. Hoffmann, Ph.D., F.R.S. Received Nov. 17, 1853.

The author refers to a former memoir in which he described the mode of preparation and properties of two new organic acids, the analogues of benzoic and nitrobenzoic acids in the toluyl or immediately succeeding series, and to which the names of toluylic ($C_{16}\, H_8\, O_{44}$) and nitrotoluylic ($C_{16}\, H_7\, (NO_4)\, O_4$) acids were consequently given.

In the present paper he resumes the study of the action of nitric acid on cymol ($C_{20}\, H_{14}$), and describes first some unsuccessful attempts to procure from that oil the substitution compound $C_{20}\left\{\begin{matrix}H_{13}\\NO_4\end{matrix}\right\}$, from which, by the action of reducing agents, he had hoped to procure a new organic base homologous with aniline, toluidine, &c. He then investigates the products of the decomposition of his new nitrogen acid. He describes the preparation and properties of nitrotoluylamide $C_{16}\left\{\begin{matrix}H_6\\NO_4\end{matrix}\right\}O_4,\ NH_2$, and having succeeded, though by a rather tedious process, in obtaining this substance in some quantity, he studies the action of reducing agents on it. By the action of hydrosulphate of ammonia upon an aqueous solution of the amide, a crystalline substance was procured, which analysis proved to be homologous with the *carbamide—carbanilide* of Hofmann, and with the *anilo-urea* of Chancel. The study of its properties showed that it must be considered as the analogue of the latter, that it is the true urea of the toluyl series, being a well-defined organic base, forming a series of crystalline salts, of which the nitrate and oxalate were qualitatively examined. A synoptical view of these ureas is given, showing their relation with the urea type.

By the action of a boiling solution of caustic potash on toluyl urea ($C_2\, H_3\, (C_{14}\, H_7)\, N_2\, O_2$), a new acid was procured, the analysis of which showed that it has three homologues in the benzoyl series, viz. *anthranilic acid, benzamic acid,* and *carbanilic acid,* all of which are represented by the formula $C_{14}\, H_7\, NO_4$, the composition of the new acid being expressed by the formula ($C_{16}\, H_9\, NO_4$).

The limited quantity of this acid at the author's disposal, and the great difficulty with which it was procured, did not enable him to decide positively with which of the above acids it corresponds, though its

mode of formation would render it probable that it is the true analogue of *carbanilic acid.* The determination of this question is of some interest, inasmuch as should it prove to correspond to *anthranilic acid,* a road might through it be opened for the introduction of a series of new substances at present entirely wanting, namely, the proper homologues of *salicylic* acid and its derivatives. The author proposes to return to this subject, and he gives, in conclusion, a synoptical view of those corresponding members of the *benzoyl* and *toluyl* groups which in the present and former paper he has established.

December 15, 1853.

THOMAS BELL, Esq., V.P. in the Chair.

The following communications were read:—

1. Extract of a Letter from Dr. Edward Vogel to Colonel Sabine, dated Mourzuk, Oct. 14, 1853.

"You will receive through the Foreign Office a packet of Astronomical, Meteorological, and Magnetical Observations, made on the way from Tripoli and since my arrival here. My instruments are almost all in good condition, although their cases have split from the effects of heat and dryness, notwithstanding their double, and in some instances triple, leather protection. I saw the great comet for the first time on August 23, but others had seen it three or four days sooner. Its nucleus was very bright, resembling a star of the first magnitude, with a distinctly defined disc of the apparent diameter of Jupiter. The tail made an angle of 86° or 87° with the horizon, inclining to the north. It was a single tail with almost precisely parallel sides. Its length was 10° on the 25th of August, 12° on the 26th, and 15° on the 27th and 29th of August. I have seen here repeatedly the apparent fluctuation in the position of stars which is spoken of in the third volume of Cosmos, and have sent the particulars of my observations to Baron von Humboldt. There is no regular rainy season at Mourzuk, but slight showers occur sometimes in the winter and spring, seldom in the autumn. A heavy rain is considered a great calamity, as it destroys all the houses, which are built of mud dried in the sun. It would likewise kill the date trees, by dissolving the salt which is in large quantities in the soil. About twelve years ago there perished above 10,000 date trees in the neighbourhood of Mourzuk, on account of a rain which continued for seven days. The prevailing winds are south and east, the strongest generally west or north-west. Twice or three times I have seen whirlwinds pass through the town, a phenomenon which was common in the deserts between Benioleed and Mourzuk. All the whirlwinds I observed turned in the direction from east by north and west to south. In December and the first half of January the thermometer falls at sunrise (at Moorzuk) as low as 42°, and in places exposed to the wind water freezes during the night. At Sokna I found no one who could remember having seen snow; but at Ghadamis snow was seen by Mr. F. Warrington on the 15th of

January 1851. At Tripoli we had heavy dews at night; and I observed the same until we had passed a small chain of mountains fifteen miles north of Sokna; from thence we had no dew, and it was even often impossible to get the dew-point with Daniell's hygrometer. In the desert the thermometer generally rose till 4 P.M., from the sand (which was sometimes heated to 140° at 1 P.M.) giving out its heat. Earthquakes are unknown in Fezzan; slight shocks are sometimes felt at Benioleed and Sokna, as was the case the end of last May. Shooting stars were observed in great quantities (about forty an hour) on the 7th, 8th, and 31st of July; very few on the evenings of the 9th, 10th, and 11th of August, averaging fifteen an hour, mostly coming from Cassiopea and Ursa Minor. On the 10th, at 8 A.M., I saw in ten minutes three shooting stars coming from α Cassiopeæ, and rising right upwards towards the zenith. About 4 A.M. on the 11th, I observed in a quarter of an hour about twenty very bright ones in Pegasus and Aries. Shooting stars were numerous also on the 1st, 2nd and 3rd of October."

2. Notice of a Comet seen from H.M. Brig Penguin off the Coast of South Africa.

"Sept. 29, 1853.

"Sir,—I am not aware that it can be of any service, still I have thought proper to write you, for the information of the Royal Society, that on the 1st of May 1853, on board H.M. brig Penguin, in lat. 35° 0′ S., long. 21° 52′ E., at $6^h\ 30^m$ P.M. I observed a comet bearing N.W. by W. $\frac{1}{4}$ W., measuring from the centre star in the belt of Orion 14° 30′, the altitude of the comet being 26° 19′, its length being about 5°. It went down at $9^h\ 10^m$, bearing W. $\frac{1}{4}$ N., and from the altitude, time, and rate it appeared to move at, it must have been the first evening of being seen. On first observing it, it appeared to be making a retrograde motion, or tail first, and not travelling as fast as the two small stars above it, as by the time it set it had approached very close to them: the weather fine, warm, and cloudy. On the 3rd of May a gale came on which lasted till the 7th, after that time it became rather indistinct, not being seen but when very clear, or by the aid of a glass. Having sailed on the 29th April from Simon's Bay, Cape of Good Hope, and not hearing there, or seeing anything of a comet, in either the Nautical or Cape Almanacs, I concluded it had not as yet been observed, and therefore have thought it my duty to forward a rough sketch of its appearance on the days mentioned, and its positions, as near as I could place it with my left hand, my right unfortunately having been partially smashed and disabled on the night the gale commenced, and which also prevented me from measuring its distance from any of the stars, &c. A copy of the latitude, longitude, and bearings is from the ship's log. I found on our arrival at Quilemane that it had been observed by the other two cruisers, but not till the 6th of May. I must apologize for troubling you with the above, but considering it as a point of duty that I should do so, I have the honour to be,

"Sir, your very obedient Servant,

"W. B. EDWARDS,
"Master H.M. Brig Penguin."

"*The Secretary,*
Royal Society."

3. "On a New Method of propagating Plants." By E. J. Lowe, Esq., F.R.A.S., F.G.S. &c. Received Nov. 17, 1853.

The author states that the experiment of a new method of propagating plants has been so successful, that he has taken the liberty of forwarding to the Royal Society this short paper upon the subject, for the guidance of those who are interested in the advance of horticulture.

It had occurred to him, that if a cutting of a plant were sealed at the base, so as to exclude the moisture of the soil from ascending the stem in injurious quantities, the method of striking cuttings of most species of plants would not be so precarious a process as at present; and accordingly some collodion was obtained in order to make the experiment.

With respect to this new process, he states, that immediately upon the cutting being severed from the parent stem, the collodion was applied to the wound, and then left a few seconds to dry, after which the cuttings were potted in the ordinary manner.

To test the value of this new process more effectually, duplicates of all the species experimented upon were at the same time similarly planted, without the collodion being applied to them.

Experiments were carried on in two different ways; one batch of cuttings being placed on a hot-bed, whilst a second batch was planted in the open ground, without even the protection of glass.

First Batch.—All of which were placed on a hot-bed on the 1st of September, and examined on the 1st of October:—

Stove Plants.

Number of cuttings with collodion applied.	Name of plant.	Number of cuttings which took root.	Number of cuttings without the application of collodion.	Number of cuttings which took root.
1	Ixora coccinea	1	1	0
1	Tacsonia manicata	1	1	1
3	Franciscea Hopeana	3	3	0
3	Franciscea Pohliana	3	3	0
2	Gloxinia Maria van Houtte	0	2	1
2	Begonia incarnata	2	2	1
8	Achimenes patens	7	8	6
2	Hoya bella	2	2	1
2	Rondeletia speciosa	2	2	1
2	Allamanda nerifolia	2	2	1
	Greenhouse Plants.			
6	Boronia serrulata	5	6	0
3	Polygala dalmaisiana	1	3	0
6	Polygala grandiflora	3	6	2
6	Verbena luna	6	6	6
1	Chorozema cordata	1	1	0
1	Epacris pallida	0	1	0
2	Leschenaultia formosa	2	2	1
1	Swainsonia astragalifolia	1	1	0
1	Swainsonia galegifolia	0	1	0
2	Abelia rupestris	2	2	0
4	Plectranthus concolor, picta	2	4	2

Second Batch.—Planted in the open ground on the 1st of September, and examined on the 1st of October:—

Hardy Plants.

Number of cuttings with collodion applied.	Name of plant.	Number of cuttings which took root.	Number of cuttings without the application of collodion.	Number of cuttings which took root.
12	Garrya elliptica	5	12	1
12	Erica vagans............................	7	12	4
18	Bupleurum longifolium	6	18	0
12	Laurus fœtens	10	12	7
6	Rose, Souvenir de la Malmaison...	4	6	3
12	Taxus baccata, golden-leaved var.	8	12	4

	Total number of cuttings to which collodion was applied.	Number of cuttings which took root.	Total number of cuttings without the application of collodion.	Number of cuttings which took root.
First batch	59	46	59	23
Second batch ...	72	40	· 72	19

The experiment, the author considers, speaks for itself. Notwithstanding the season being too far advanced for the full benefit of the process to be thoroughly observed, still twice as many cuttings took root treated by the new method as had rooted by the old. The mortality in the open ground was increased by slugs having eaten off above the soil some of the cuttings; those thus damaged were examined after they had been in the ground a month, and it was found that the collodion was quite as sound as when first applied. It would therefore appear that the collodion seals the wound of the cutting, and protects it from the fatal effects of damp, until roots are prepared to force through the covering of gun-cotton. It is further stated, that the application of this solution has been found to be exceedingly beneficial in the pruning of such plants as *Euphorbia speciosa*, *Impatiens latifolia*, *Impatiens latifolia-alba*, *Hoya bella*, *Hoya imperialis*, &c., the cut branches being prevented from bleeding.

It is the author's intention next spring to follow out this experiment, in budding and grafting, as he considers that it will also be useful in this branch of horticulture.

Gutta-percha, dissolved in æther, was in some instances substituted to heal the wounds caused by pruning; yet owing to this solution not drying as rapidly as collodion, the first, and sometimes the second application was not sufficient.

The effect of these solutions upon cut flowers was very marked. Two branches were gathered as nearly alike as possible; to the flower-stalks of the one, collodion was applied. These flowers were placed in vases filled with water; those coated over with collodion began to fade in thirty-six hours, and many were quite

dead in three days; whilst the flowers merely placed in water in the ordinary manner remained fresh and healthy. Those that faded soonest were *Reseda odorata* and *Tropæolum majus*, and those which were least affected were *Tagetes erecta* and *Senecio erubescens*.

4. "On the Acidity, Sweetness, and Strength of Wine, Beer and Spirits." By H. Bence Jones, M.D., F.R.S. Received Nov. 17, 1853.

(1.) The acidity of the different liquids was determined by means of a standard solution of caustic soda. The quantity of liquid neutralized was always equal in bulk to 1000 grs. of water at 60° F.

The acidity in different—

Sherries	varied from	1·95 grs.	to	2·85 grs.	of caustic soda.
Madeira	,,	2·70	,,	3·60	,,
Port	,,	2·10	,,	2·55	,,
Claret	,,	2·55	,,	3·45	,,
Burgundy	,,	2·55	,,	4·05	,,
Champagne	,,	2·40	,,	3·15	,,
Rhine wine	,,	3·15	,,	3·60	,,
Moselle	,,	2·85	,,	4·50	,,
Brandy	,,	0·15	,,	0·60	,,
Rum	,,	0·15	,,	0·30	,,
Geneva	,,	0·07			,,
Whisky	,,	0·07			,,
Bitter ale	,,	0·90	,,	1·65	,,
Porter	,,	1·80	,,	2·10	,,
Stout	,,	1·35	,,	2·25	,,
Cider	,,	1·85	,,	3·90	,,

Hence the order in which these wines may be arranged, beginning with the least acid, is Sherry, Port, Champagne, Claret, Madeira, Burgundy, Rhine, Moselle.

(2.) The sugar was determined by means of Soleil's saccharometer, which at least gives the lowest limit to the amount of sugar.

The sweetness in different—

Sherries	varied from	4 grs.	to	18 grs.	in the ounce.
Madeira	,,	6	,,	20	,,
Champagne	,,	6	,,	28	,,
Port	,,	16	,,	34	,,
Malmsy	,,	56	,,	66	,,
Tokay	,,	74			,,
Samos	,,	88			,,
Paxarette	,,	94			,,

Claret, Burgundy, Rhine, and Moselle contained no sugar.

Hence the order in which these wines may be arranged, beginning with the driest, is—

Claret Burgundy Rhine Moselle
Sherry
Madeira
Champagne
Port
Malmsy
Tokay
Samos
Paxarette.

In a dietetic view, assuming that the sugar becomes acid, then the mean results as to the acidity of the different fluids examined, beginning with the least acid, is—

Geneva Whisky
Rum
Brandy
Claret
Burgundy
Rhine wine
Moselle
Sherry
Madeira
Champagne
Cider
Port
Porter
Stout
Malmsy Madeira
Ale
Tokay.

(3.) The alcohol was determined by means of the alcoholometer of M. Geisler of Bonn.

The strength of different samples of—

Port	varied from	20·7	per cent. to	23·2	per cent. by measure.
Sherry	,,	15·4	,,	24·7	,,
Madeira	,,	19·0	,,	19·7	,,
Marsala	,,	19·9	,,	21·1	,,
Claret	,,	9·1	,,	11·1	,,
Burgundy	,,	10·1	,,	13·2	,,
Rhine wine	,,	9·5	,,	13·0	,,
Moselle	,,	8·7	,,	9·4	,,
Champagne	,,	14·1	,,	14·8	,,
Brandy	,,	50·4	,,	53·8	,,
Rum	,,	72·0	,,	77·1	,,
Geneva	,,	49·4			,,
Whisky	,,	59·3			,,
Cider	,,	5·4	,,	7·5	,,
Bitter ale	,,	6·6	,,	12·3	,,
Porter	,,	6·5	,,	7·0	,,
Stout	,,	6·5	,,	7·9	,,

The Burgundy and Claret have less alcohol than was found by Mr. Brande forty years ago in the wines he examined. The Sherry is now stronger, the Port is not so strong, the Marsala is weaker, the Rhine wine is the same strength, the Brandy is as strong as formerly; the Rum is nearly half as strong again; the Porter is stronger, and the Stout rather stronger than formerly.

Lastly, the specific gravity of each liquid was taken. As this however chiefly depends on the amount of alcohol and sugar present, and as these were directly determined, the specific gravity may be taken as a distant control on the amount of sugar present.

Thus, in those wines in which the amount of alcohol was the same, the specific gravity was found to vary with the amount of sugar found by the saccharometer.

The results of the analysis of each sample of wine, &c. is given in a series of tables, which do not admit of any abstract.

December 22, 1853.

THOMAS GRAHAM, Esq., V.P., in the Chair.

The following papers were read:—

1. "An Inquiry into some of the circumstances and principles which regulate the production of Pictures on the Retina of the Human Eye, with their measure of endurance, their Colours and Changes." By the Rev. W. Scoresby, F.R.S., Corresponding Member of the Institute of France, &c. Received Nov. 19, 1853.

The investigations of the author embrace three distinct cases,—the case of achromatic pictures; that of coloured pictures of uncoloured objects, derived simply or mainly from the influence of light on the eye; and that of the spectra of coloured objects, together with certain applications of the results obtained to other optical characteristics, determinations or phenomena.

The general mode of experiment employed in these researches is described as "the viewing of illuminated objects with a steady fixed gaze at a special point, and then determining the impression on the retina by examining the images developed *with closed eyes*." The time of viewing the objects varied from a momentary glance up to half a minute, more rarely to a minute; and the mode of eliciting the impression was, primarily, by closing the eyelids into gentlest contact, whilst the head was kept unmoved, and the eyelids steady in their original direction. Thus performed, the experiment becomes very simple and manageable, and the results, various as they are in colour or depth of tint, are almost unfailingly elicited and often curious or beautiful.

Whilst the general result of viewing an illuminated object is the production of a clearly-defined picture on the retina, appearing in certain cases instantly, or more commonly, from 3 to 5 seconds after the eyes are closed,—the nature or quality of the picture, with its

degree of endurance and changes, is found to present, under differences in the circumstances, an almost endless variety. Thus the results, it was found, might be varied by differences in the time of gazing on the object; by differences in the intensity of the external light, and by the partial or total exclusion of the light of the room from the eyelids; by alterations in the degree of compression of the eyelids; by the movement of the eyeballs during the time of observing the picture; as also by variations in the normal state of the eyes on commencing the experiments.

All these influencing circumstances had been made successively, or sometimes combinedly, the subjects of special investigation by the author; and ultimately, in most respects, he considered, so far as his own eyes might be deemed to yield *general* phenomena, with satisfactory or conclusive results. Various experiments had been made on the spectra derived from light reflected from opake objects in comparison with those elicited by light transmitted by transparent substances, both white and coloured; as also on the differences in the measure of endurance, the variety of their repetitions, and the phenomena of their changes in colour, of the pictures photographed within the eye, under curiously modified conditions.

The present communication, however, comprises only a part of these investigations, the first of the cases referred to at the outset, viz. inquiries respecting *colourless pictures* on the retina, derived from the viewing of objects under low or moderate degrees of light, or of pictures observed irrespective of chromatic effects.

1. As to the effect of *Time* in the viewing of an illuminated object, on the nature and permanency of the picture produced, it was found that, in favourable states of the eyes, a mere momentary glance (such as of a window viewed from the back of a room) was sufficient for producing a distinct negative picture of the illuminated aperture, with the cross bars of the window-frame, which, under certain changes, could be seen ordinarily for about 20 seconds, and under strong light, sometimes for an interval of a minute or two of time, if not more. But the impression from a continuous viewing of a window rather strongly illuminated, for a period of a minute, was very remarkable, the image remaining on the retina whilst the experimenter was breakfasting, and also engaged in writing, so as to be distinctly seen, on slightly closing the eyes, an hour afterwards, and, in another case which he particularly describes, after a lapse of 80 minutes.

2. Experiments on the effects of *quantity* or *intensity* of light, on the visual spectra derived from uncoloured objects, showed that such spectra were yielded by extremely low degrees of illumination. The light, for instance, of the moon or stars thrown on a white linen blind, produced distinct negative pictures of the slightly illuminated aperture. Candlelight gave also negative pictures of white and black objects. Low illumination from transmitted solar light gave, in most cases, colourless pictures, appearing sometimes immediately on closing the eyes, as by a flash of light, or otherwise in 3 to 5 seconds in negative tints; these pictures, where the object had

been viewed for some seconds, were found to fade away and subsequently reappear in less dark shades, sometimes with several such changes.

3. The changes in the optical spectra from the *partial* or *entire exclusion of light* from the closed eyelids were found to be very striking. No matter how this diminution or exclusion of light was effected,—whether by the thickening of the eyelids by compression, or turning the face away from the light, or interposing the hand or other opake substance betwixt the eyes and the light, or covering the face altogether,—the spectra assumed a new character as to light and shadow, ordinarily, but not in all cases, complementary to the tints originally observed. A total exclusion of the external light still left the picture clear and distinct, with a continuance, after occasional changes, little differing from that of other experiments.

4. This measure of fixidity of the spectra impressed on the retina led the author to some curious results in obtaining duplicate or *multiple pictures* of the same object. Thus, by gazing at a window, successively at different fixed points previously determined on, he multiplied the cross bars so as to produce a picture of a window with twice or quadruple the number of panes. A white statuette, viewed at different points in succession, whilst strongly illuminated, enabled the author to obtain double pictures in black or grey, associated according to the relation of the points gazed at, in unlimited variety. Or viewing the statuette from two positions differing in distance, he obtained images of different dimensions. Double images, too, were obtained by using the eyes separately; and also by looking at an object nearer to the eyes than the statuette, so that the lines of the axes might diverge at the distance of the statuette, thus beautifully elucidating one of the chief causes of the indistinctness of vision as to objects nearer to, or more remote from the eyes than that directly contemplated.

5. Complete pictures were also obtained by the *combination of parts* separately viewed, whilst various impressions, however incongruous, were combined into one picture. Thus parts of the statuette were viewed, under the adoption of a moveable screen, so as either to combine the separately-viewed portions rightly, or to transfer one part, such as the head, to either shoulder, or to adjust two heads in different positions. Separate impressions, also, of segments of the statuette were taken on the eyes singly, and these combined, accordingly as the same or different points of view were selected, into perfect or distorted pictures. The appearance of the parts of the resulting spectrum, however, were not always synchronous portions, sometimes appearing and disappearing by separate or partial changes, like the effect of the dissolving views of the magic lantern.

6. Pictures, diagrams, printing, &c., were found, under due influence of light, to yield cognizable and sometimes vivid impressions on the retina. Diagrams in black and white, or chequered surfaces like that of a chess-board, gave very distinct pictures, always negative, the squares coming successively into view, beginning with the portion gazed at. The succession of changes, when the impression

was strong, in this experiment was not a little curious, the perfect image of the chess-board after bursting into view, gradually fading altogether away, and then reviving, in less strong tints, in a series of repetitions.

Another curious, though anticipated result, the author also describes under this section,—the determination, by viewing the ocular spectra, of portions of diagrams or elements in pictorial or typographical surfaces, which had not been noticed in the act of gazing. Thus, particularly on viewing a line of printed figures at a particular point, without noticing those on either side, a considerable series, right and left, were so plainly depicted on the visual organ as to be easily known; whilst, in like manner, a point in a line of a printed placard being gazed at, the lines above and below came into view on closing the eyes, and could frequently be read.

Of certain general facts elicited by this first series of investigations, the author notices, that in viewing impressions on the retina with closed eyes, all the pictures appear to occupy a position *externally*, similar to the effect when the objects are directly seen; that the spectra derived from moderate or strong degrees of transmitted light have prevalently the character of transparency, and those from very low degrees, most ordinarily, of opacity; that although many of the spectral phenomena the author had observed were well known to be capable of elicitation in the ordinary form of the experiment with the eyes open, yet the series of phenomena, as a whole, could not be so elicited, nor was it possible by such form of experiment to analyse and compare the phenomena whilst in progress of change, which, in the form he had adopted, were usually exhibited as plainly as if the spectra were the real and immediate effects of ordinary direct vision; and that such is the precision and such the certainty with which the pictures are ordinarily developed, after duly viewing any illuminated object, that the expected result, so far as the eliciting of definite pictures is concerned, hardly ever fails.

2. "On certain Properties of Square Numbers and other Quadratic Forms, with a Table by which all the odd numbers up to 9211 may be resolved into not exceeding four square numbers." By Sir Frederick Pollock, F.R.S. &c. Received Dec. 20, 1853.

In examining the properties of the triangular numbers 0, 1, 3, 6, 10, &c., the author observed that every triangular number was composed of four triangular numbers, viz. three times a triangular number plus the one above it or below it; and he found that all the natural numbers in the interval between any *two* consecutive triangular numbers might be composed of four triangular numbers having the sum of their roots, or rather of the indices of their distances from the first term of the series constant, viz. the sum of the indices of the four triangular numbers which compose the first triangular number of the *two*.

Not being at that time aware of any law by which the series that fills up the intervals could be continued, he subsequently turned his attention to the square numbers as apparently presenting a greater

variety of theorems. He observed that if any four square numbers, a^2, b^2, c^2, d^2, have their roots such, that, by making one or more positive and the rest negative, the sum of the roots may be equal to 1, then if the root or roots of which the sum is 1 less be each of them increased by 1, and the others or other be each diminished by 1, the sum of the squares of the roots thus increased or diminished will be $a^2+b^2+c^2+d^2+2$. This he found to be only a particular case of more general theorems.

Theorem A.—If the sum of the roots a, b, c, $d=2n-1$, and n be added to each of the less set, and subtracted from each of the greater, the *increase* in the sum of the squares of the new roots will be $2n$.

Theorem B.—If the sum of the roots $=2n+1$, and n be added to each of the less set and subtracted from each of the greater, the *diminution* in the sum of the squares of the new roots will be $2n$.

By means of these he shows—

Theorem C.—If any four squares be assumed which compose an odd number, these may be diminished till four squares are attained the sum of whose roots will equal 1.

By applying the first of these theorems to four roots, the sum of whose squares is an odd number, the author deduces, in a tabular form, the squares (four or less) which compose the odd numbers from 21 to 87; and remarks that there does not appear to be any limit to this mode of continuing to increase the sum of four squares by 2 each time. As, however, although this may render it probable that every odd number is composed of four, three, or two squares, it falls very short of a mathematical proof, unless it can be shown that the series can be continued by some inherent property belonging to it, he proceeds to examine the series, in order to ascertain what approach can be made to such a proof.

Adopting a method similar to that observed in the triangular numbers, the author forms what he terms the series of *Gradation*, by means of which the series of squares which compose the odd numbers may be advanced by steps or stages which increase regularly and obey a certain law, and at which this series is, as it were, commenced anew from roots of the form $n, n, n, n+1$, or $n-1, n, n, n$; the form of the sum of the squares of these roots being $4n^2 \pm 2n+1$, and the series of gradation 1, 3, 7, 13, 21, 31, 43, 57, 73, &c. On this principle a more extended table of the odd numbers resolved into squares (not exceeding four in number) is constructed. On this the author remarks that it is complete to the 96th odd number (191), that is, there are in this table square numbers which will form the odd numbers in succession, whose roots (some $+$, some $-$) $=1$; and therefore the expression $4n^2 \pm 2n+1$ up to $4n^2 \pm 2n+191$ may be divided into 4 or 3 squares, whatever be the value of n. The numbers in the table exactly fill up the interval between

$$47^2,\ 47^2,\ 47^2,\ 48^2=8931,$$

and

$$47^2,\ 48^2,\ 48^2,\ 48^2=9121,$$

whose difference $=190$, the difference between the first term and

the last term in the table: it will therefore resolve into square numbers any odd number up to $9121+190=9211$.

With reference to the mode in which the intervals in the table may be filled up, the author states the following general theorems relating to the sums of three square numbers, by means of which the roots may be varied, and yet the sum of the squares remain the same.

Theorem D.—If any three terms of an arithmetical series, and omitting the 4th term, the three following terms be arranged thus,

$$\begin{array}{ccc} a+b, & a+2b, & a+6b, \\ a\ \ , & a+4b, & a+5b, \end{array}$$

the sum of the squares of each set of terms will be the same.

Theorem E.—If four numbers in arithmetical progression be placed thus,

$$\begin{array}{cc} a\ \ , & a+2b, \\ a+4b, & a+6b, \end{array}$$

and the sum of the 1st and 4th be divided into two parts whose difference shall be four times the arithmetic ratio, as $a+7b-(a-b)$, and the parts be placed with the terms, the greater with the less, and the less with the greater, thus,

$$\begin{array}{ccc} a\ \ , & a+2b, & a+7b, \\ a-b, & a+4b, & a+6b, \end{array}$$

the sum of the squares will be equal.

Theorem F.—Let two numbers which differ by $2n$ be placed thus:

$$\begin{array}{cc} a+n, & a+n, \\ a-n, & a-n, \end{array}$$

then if the sum of the four ($4a$) be divided so as to have the same difference ($2n$), and the parts be placed, the less with the greater, and the greater with the less, thus,

$$\begin{array}{ccc} a+n, & a+n, & 2a-n, \\ a-n, & a-n, & 2a+n, \end{array}$$

the sum of the squares shall be the same.

The author illustrates this part of the subject by deducing six forms of roots whose squares $=197$.

January 12, 1854.

The LORD CHIEF BARON, V.P., in the Chair.

Commander Kay, R.N., was admitted into the Society.

A paper was read, entitled "On some New and Simple Methods of detecting Manganese in Natural and Artificial Compounds, and

of obtaining its Combinations for œconomical or other uses." By Edmund Davy, Esq., F.R.S., M.R.I.A. &c. Received December 4, 1853.

In this paper the growing importance of manganese since its discovery, and its extensive distribution in Nature are noticed. Manganese is chiefly found combined with oxygen, but its oxides are commonly mixed with those of iron, and though different methods of separating them have been recommended, yet no very simple or unobjectionable test for manganese seems to be known. Two methods for detecting manganese are recommended, viz.—

1. The pure hydrated fixed alkalies, potash and soda, and especially potash. 2. Sulphur.

With regard to the first method. Though the compound *Chameleon mineral* made by strongly heating nitre or potash and peroxide of manganese together, has long been known, yet it appears hitherto to have escaped observation, that potash seems to be a more delicate test of manganese than any other known substance. The use of potash in this way is simple and easy; it is employed in solution; equal weights of the alkali and water form a fluid well-adapted for the purpose; different metals may be used in the form of slips on which to make the experiments, but a preference is given to silver foil, as it is less acted on by alkalies than platina, and is more readily cleaned. A slip of such foil, about two or three inches in length and half an inch wide, answers well. Solids, to be examined for manganese, are finely pulverized; fluids require no preparation; the smallest portion of either is mixed with a drop or part of a drop of the alkali on the foil and heated by a spirit-lamp (for many experiments a candle affords sufficient heat), when on boiling the alkali to dryness and raising the heat, the characteristic green manganate of potash will appear on the foil. The delicacy of the alkali as a test thus applied, will be obvious on using the most minute portions of manganese ores in fine powder, and the author's son, Dr. E. W. Davy, readily detected manganese in a single drop of a solution containing one grain of solid sulphate in ten thousand grains of water. The presence of other oxides do not appear to impair the efficacy of this test. A strong solution of hydrate of soda in water, used in a similar manner, affords an excellent test for manganese, little inferior in delicacy to potash, but the latter is shown to be preferable.

Carbonate of soda has long been regarded as one of the most delicate tests of manganese, especially if aided by a little nitrate or chlorate of potash, but that carbonate is much inferior as a test for manganese to potash or soda, requiring a far higher temperature to form the manganate of soda, and the aid of oxidizing substances, as nitre and chlorate of potash, which are quite unnecessary with those alkalies. Borax, too, in point of delicacy is not to be compared with the fixed alkalies as a test for manganese.

The author is of opinion that the fixed alkalies in solution and silver foil will form a valuable addition to the agents employed by the mineralogist and chemist in the examination of minerals, ores, &c.

2. *Sulphur*.—If a little flowers of sulphur be mixed with about its own bulk of the common peroxide of manganese, and exposed on a slip of platinum foil to a red heat, sesquioxide, sulphuret and sulphate of manganese will be formed, and by continuing the heat for a short time, an additional quantity of the sulphate will be produced from the sulphuret. On treating the mass with water and filtering the fluid, a solution of sulphate of manganese will be obtained which will yield a white precipitate with the ferrocyanide of potassium, without a trace of iron.

Similar experiments may be made with any manganese ores, or with substances known or suspected to contain manganese. The quantity of materials operated on may be increased or diminished at pleasure; but if increased, the heat should be continued a little longer, to decompose any remaining sulphuret, and thus add to the quantity of sulphate formed. In the same way manganese was detected in some minerals in which it was known to exist, and in others in which it had not been previously found; likewise in soils and subsoils, in the ashes of coal and peat, in a number of pigments, and also in the ashes of different fabrics partially dyed brown by manganese.

Sulphate of manganese is formed, with sulphuret, when sulphurous acid gas is made by heating a mixture of peroxide of manganese and flowers of sulphur, even in close vessels. The sulphate may also be more readily obtained, in quantity, by simply boiling a solution of common green vitriol in water for about a quarter of an hour or upwards, in contact with an excess of sesquioxide of manganese in fine powder, till the solution affords a white precipitate with ferrocyanide of potassium.

Chloride of manganese may also be formed in a similar manner by boiling an aqueous solution of protochloride of iron with an excess of sesquioxide of manganese, or it may be made with greater facility by dissolving this oxide in the common muriatic acid of commerce, taking care that the oxide be present in excess.

The brown sesquioxide of manganese may be made, not only by means of sulphur, but more readily and better by mixing the common peroxide with about one-third of its weight of peat mould, sawdust or starch, and exposure to a red heat in an open crucible with occasional stirring for about a quarter of an hour, or until the oxide acquires a uniform brown colour.

The sulphate and chloride of manganese being extensively used in dyeing, calico-printing and other arts, and in making the compounds of manganese, the simple means stated of forming those salts, free from iron (it is presumed), are material improvements on the circuitous methods hitherto adopted.

The following was also read:—

Supplement to a paper "On certain Properties of Square Numbers and other Quadratic Forms, with a Table, &c." By Sir Frederick Pollock, F.R.S. &c. Received Jan. 9, 1854.

In the original draft of this paper there was a suggestion that all the terms of the series 1, 3, 7, 13, &c. [there called the Gradation-

Series] possessed the property that was exhibited as belonging to the odd number 197. This was omitted in the copy from some doubt whether it was universally true. Since the paper was read that doubt has been removed, and it turns out that the property belongs not only to *all* the terms of the series 1, 3, 7, 13, &c., but to all odd numbers whatsoever. I am desirous to add to the paper this statement by way of supplement. The property referred to may be thus enunciated:—

Every odd number may be divided into square numbers (not exceeding 4) whose roots (positive or negative) will by their sum or difference [in some form of the roots] give every odd number from 1 to the greatest sum of the roots, which (of course) must always be an odd number.

Or the theorem may be stated in a purely algebraical form, thus:—If there be two equations

$$a^2+b^2+c^2+d^2=2n+1$$
$$a+b+c+d=2r+1,$$

a, b, c, d being each integral or zero, n and r being positive, and r a maximum; then if any positive integer r' (not greater than r) be assumed, it will always be possible to satisfy the pair of equations

$$w^2+x^2+y^2+z^2=2n+1$$
$$w+x+y+z=2r'+1$$

by integral values (positive, negative or zero) of w, x, y, z.

I hope shortly to communicate a proof of the above theorem, independent of any of the usual modes of proving that every odd number is composed of (not exceeding) four square numbers.

Note.—The differences of the roots of 197 were not fully stated in the paper, I add them here:—

197

has 7 forms of roots:—

Forms of roots.	
14, 1, 0, 0	
12, 7, 2, 0	3 ... 7 .... 17 21
12, 6, 4, 1	1, 3 ...... 9, 11, 13 19, 21, 23,
11, 6, 6, 2	1, 3 ...... 9, 13, 21, 25,
10, 9, 4, 0	3, 5, ... 15, ... 23
10, 6, 6, 5	3, 5, 7 .... 15, 17, ... 27.
9, 8, 6, 4	1, 3, 7 11, 15, 19 .... 27.

January 19, 1854.

CHARLES WHEATSTONE, Esq., V.P., in the Chair.

A paper was read, entitled "On the Geometrical Representation of the Expansive Action of Heat, and the Theory of Thermo-dynamic Engines." By William John Macquorn Rankine, Civil Engineer, F.R.S.S. Lond. and Edinb. &c. Received December 5, 1853.

The author remarks, that if abscissæ be measured from an origin

of rectangular coordinates, representing the volumes assumed by an elastic substance, and if ordinates, at right angles to those abscissæ, be taken to denote the corresponding expansive pressures exerted by the substance, then any succession of changes of pressure and volume may be represented geometrically by the coordinates of a curve. If such a curve have two extremities, the area included between the curve and the ordinates let fall from its extremities will represent (when positive) the expansive power given out by the substance during the process represented by the curve. Should the curve be closed, returning into itself, so as to denote a cycle of periodical changes of pressure and volume, then will the area, enclosed within the curve, represent (when positive) the expansive power given out during one cycle of changes. This area is positive when increase of volume takes place on the whole at greater pressures than diminution of volume. The area of such a closed curve represents also (when positive) the mechanical equivalent of the heat which permanently disappears, or is converted into expansive power, during a cycle of changes, for were it not so, the sum of energy in the universe would be changed, which is impossible.

As the principles of the expansive action of heat are capable of being presented to the mind more clearly by the aid of diagrams of energy than by means of words and symbols alone, such diagrams are applied, in the present paper, partly to the illustration and demonstration of propositions previously proved by other means, but chiefly to the solution of new questions, especially those relating to the theory of thermo-dynamic engines.

Throughout the whole of this paper, quantities of heat are expressed, not by units of temperature in an unit of weight of water, but by equivalent quantities of mechanical power, stated in foot-pounds according to the ratio established by Mr. Joule's experiments on friction (Phil. Trans. 1850), that is to say, 772 foot-pounds per degree of Fahrenheit, or 1389·6 foot-pounds per Centigrade degree, applied to one pound of liquid water at atmospheric temperatures.

A curve described on a diagram of energy, such that its ordinates represent the pressures of a homogeneous substance corresponding to various volumes of an unit of weight, while the total sensible or actual heat (Q) present in an unit of weight of the substance, is maintained at a constant value (Q_1), may be called the *Isothermal Curve* of Q_1 for the given substance. Its equation is

$$Q=Q_1.$$

If an unit of weight of a substance be allowed to expand, under a pressure equal to its own elasticity, without receiving or emitting heat, its actual heat will diminish during the expansion, and its pressure will diminish more rapidly than it would do if the actual heat were maintained constant. A curve whose coordinates represent this mode of variation of pressure and volume may be called a *Curve of no Transmission of Heat*. For every such curve a certain function of pressure, volume and actual heat, called a *Thermo-dynamic Function* (F), has a constant value (F_A) proper to the parti-

cular curve under consideration; whose equation is therefore

$$F=F_A.$$

A curve whose coordinates represent the relation between pressure and volume when the substance is absolutely destitute of heat, may be called the *Curve of Absolute Cold.* It is at once an isothermal curve and a curve of no transmission, and is an asymptote to all the other curves of both those kinds, which approach it indefinitely as the substance expands without limit.

The whole theory of the expansive action of heat is comprehended in the geometrical properties and mutual relations of those two classes of curves; and all those properties and relations are the consequences of and are virtually expressed by the two following theorems:—

Theorem I.—*The mechanical equivalent of the heat absorbed or given out by a substance in passing from one given state as to pressure and volume to another given state, through a series of states represented by the coordinates of a given curve on a diagram of energy, is represented by the area included between the given curve and two curves of no transmission drawn from its extremities, and indefinitely prolonged in the direction representing increase of volume.*

Theorem II.—*If across any pair of curves of no transmission on a diagram of energy there be drawn any series of isothermal curves at intervals corresponding to equal differences of actual heat, the series of quadrilateral areas thus cut off from the space between the curves of no transmission will be all equal to each other.*

These two propositions are the geometrical representation of the application, to the particular case of heat and expansive power, of two axioms respecting Energy in the abstract, viz.—

Axiom I.—*The sum of energy in the Universe is unalterable.*

Axiom II.—*The effect, in causing transformation of energy, of the whole quantity of actual energy present in a substance, is the sum of the effects of all its parts.*

The application of these axioms to heat and expansive power involves the following

Definition.—*Expansive Heat is a species of actual Energy, the presence of which in a substance affects, and in general increases, its tendency to expand;*—

and this definition, arrived at by induction from experience, is the foundation of the theory of the expansive action of heat.

The first section of the paper is occupied chiefly with the demonstration of the first of the theorems quoted and its immediate consequences, which are applicable to all substances, homogeneous and heterogeneous.

The second section relates to the theory of the expansive action of heat in homogeneous substances.

From the second theorem above quoted, it is deduced, that the area of any quadrilateral bounded above and below by any two isothermal curves, and laterally by two curves of no transmission, is the product of the difference between the two quantities of actual

heat proper to the isothermal curves, by the difference between the two thermo-dynamic functions proper to the curves of no transmission, being represented by an expression of this form,

$$(Q_1-Q_2) \cdot (F_B-F_A).$$

While the area of a figure bounded above by the isothermal curve of Q_1, and laterally by the indefinitely-extended curves of no transmission corresponding to the thermo-dynamic functions F_A, F_B, is represented by

$$Q_1(F_B-F_A).$$

The area of a diagram of energy of any figure is calculated by conceiving it to be divided, by a network of isothermal curves and curves of no transmission, into an indefinite number of stripes or quadrilaterals, finding the area of each and adding them by summation or integration. By the aid of these principles various problems are solved.

In the third section the same principles are applied to determine the efficiency of thermo-dynamic engines worked by the expansion and contraction of permanent gases without and with the aid of *economisers* or *regenerators*. The *efficiency* of a thermo-dynamic engine is the proportion of the whole heat communicated to the working substance which is converted into motive power.

The *maximum theoretical efficiency* of a thermo-dynamic engine working between the limits of actual heat Q_1 and Q_2, whether without a regenerator or with a perfect regenerator, is expressed by the fraction

$$\frac{Q_1-Q_2}{Q_1}.$$

A theoretically perfect regenerator does not increase the maximum efficiency between given limits of actual heat, but merely enables that efficiency to be attained with a smaller extent of expansion, and consequently with a smaller engine.

The fourth section treats of the relation between actual heat and temperature, which must be known before the propositions of the preceding sections can be applied to actual substances. Existing experimental data are not yet adequate to the *exact* determination of this relation; but it is considered they are sufficient to show that a relation deduced by the author from the Hypothesis of Molecular Vortices (see Trans. of the Royal Society of Edinburgh, vol. xx.), is at least near enough to the truth for all purposes connected with the computation of the efficiency of thermo-dynamic engines. This relation is expressed by the formula

$$Q=\mathfrak{k}(T+T_0),$$

where T is temperature, measured from the melting-point of ice; T_0, the height of the melting-point of ice above the point of total privation of heat; and $\mathfrak{k}$, the mechanical value of the real specific heat of the substance. According to computations made in 1852

by the author from experiments by Messrs. Thomson and Joule, $T_0=272\frac{1}{2}°$ Centigrade$=490\frac{1}{2}°$ Fahrenheit, a value which may be considered sufficiently correct for practical purposes.

The maximum theoretical efficiency of every conceivable thermodynamic engine receiving heat at the temperature T_1, and giving out heat at the temperature T_2, is

$$\frac{Q_1-Q_2}{Q_1}=\frac{T_1-T_2}{T_1+T_0}.$$

The fourth section concludes with a system of formulæ, illustrated by numerical examples, for computing the power and efficiency of air-engines.

In the fifth section, the principles of the preceding sections are applied to aggregates consisting of heterogeneous substances, or of the same substance in different conditions, especially the aggregate of a liquid and its vapour; and the results are applied to the numerical computation of the theoretical efficiency of steam-engines.

Jan. 26, 1854.

The REV. BADEN POWELL, V.P., in the Chair.

1. A paper was read, entitled "On the Vibrations and Tones produced by the contact of bodies having different Temperatures." By J. Tyndall, Esq., F.R.S. Received Jan. 15, 1854.

The author introduces the subject of his paper by a brief description of the labours which have preceded his own, from the discovery of the phænomenon by Schwartz in 1805 to its revival and further examination by Trevelyan, Faraday, Forbes and Seebeck. The peculiar views of Prof. Forbes, who regards the effects as due to a new species of mechanical agency in heat, were the chief inducement to the resumption of the subject by the author. He examines the ground on which the theory of Prof. Forbes is based, and tests by experiment the general laws at which he has arrived. The first of these laws is, that "the vibrations never take place between substances of the same nature." By converting the cold metal on which the hot rocker is placed into a thin plate, fixing this plate in a vice, and causing the rocker to rest upon the edge of the plate, the author obtained vibrations with iron on iron, brass on brass, copper on copper, silver on silver, zinc on zinc, tin on tin; and thus shows the first law to be untenable. The second general law affirms that "both substances must be metallic." As exceptions to this law, the author adduces experiments made on about twenty non-metallic substances, with which perfectly distinct vibrations were obtained. Among those which signalize themselves by the force and permanence of the rockings they produce, are to be found rock-salt, fluor-spar and rock-crystal. With rockers similar to those described in the paper, and attending to the precautions there dwelt upon, vibrations and musical tones can be obtained without difficulty on these substances.

The third general law of Prof. Forbes states that "the vibrations are proportional within certain limits to the difference of the conducting powers of the metals for heat, the metal having the least conducting power being necessarily the coldest." The evidence adduced against the first law appears to destroy this one also. The author however proceeds further, and reverses the conditions deemed essential by Prof. Forbes. Silver stands at the head of conductors; using it as the *cold* metal, he has obtained distinct tones with hot rockers of brass, copper and iron, placed upon it. These and other experiments show that the third general law is, like the two others, untenable. Prof. Forbes further states that two of the metals, bismuth and antimony, are perfectly inert; the author has however obtained distinct tones with both of these substances. He finally enters also into an examination of the arguments of Prof. Forbes against the views supported by Faraday, and shows how the facts adduced against the said views become, when duly considered, strong corroborative evidence of their correctness.

1. "The following letter from Prof. Dove to the Earl of Rosse, was read from the Chair."

Berlin, Jan. 7, 1854.

My lord,—The vast stock of observations daily gathered by British Observatories for the promotion of terrestrial physics, always impressed me, as a scientific man, with gratitude towards a nation so worthy of the happy privilege of interrogating nature in every part of the globe. To day, at the receipt of the unhoped-for honour awarded to me by the Royal Society (the Copley Medal), for labours in a great measure grounded on those observations, I feel myself called upon to express a more personal, and still deeper, sense of gratitude. May I beg of your Lordship to communicate to the Council and the Society my most respectful thanks for the approbation bestowed upon the result of my exertions?

I am, my Lord,

Your Lordship's most obedient Servant,

To the Earl of Rosse, H. W. Dove.
President of the Royal Society.

2. "The following letter from Prof. Hansteen to Col. Sabine, was also read."

Observatory at Christiania, January 6, 1854.

Dear Sir,—At the end of last year I calculated formulæ of interpolation for different places in Europe, at which I had collected a sufficient number of observations of the magnetical inclination:

$$i=i_{,}+x+(t-t_0)y+(t-t_0)^2z, \quad \ldots\ldots\ldots\ldots \quad \text{(I.)}$$

where $i_{,}+x$ is the inclination for the year t_0, x, y, z constants. The annual variation is,

$$\frac{di}{dt}=y+2(t-t_0)z\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(II.)}$$

In all Europe at this time y has a *negative* value, and z a *positive* value; accordingly the variation is *decreasing*, and the dip approaching to a *minimum*. Assuming $\frac{di}{dt}=0$, and the epoch of the minimum $=\mathrm{T}$, you will find

$$\mathrm{T}=t_0-\frac{y}{2z}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(III.)}$$

As y is *negative*, the last member of T will be positive, and $\mathrm{T}>t_0$. In Siberia y is in the last 10—15 years *positive*, as well as z, and accordingly $\mathrm{T}<t_0$. When y is *positive*, and z *negative*, as in Nertchinsk and Pekin in the following table, T is the epoch of a *maximum*.

The probable error of the constants x, y, z, and T, depends on the number and goodness of the observations, the number of years between the first and last, and their more or less symmetrical distribution between these. The most doubtful in the subjoined table are marked with one or two asterisks.

In examining the table, you will remark that the epoch T of minimum in Europe will arrive earlier in the northern than in the southern places (Paris to Christiania); earlier in the eastern than in the western places (Christiania, Stockholm, Petersburg, Kazan, Catharinenburg). For the theory of the magnetism of the earth, I think it of interest to determine this epoch for different points. In order to diminish the uncertainty in different points, I have written to Professor Gedersen in Copenhagen, to Encke in Berlin, to Gauss in Göttingen, and to Kupffer in Petersburg, and entreated them to make or communicate to me observations nearer to the actual year. And it is the same solicitation I now take the liberty to address to you for London. The only seven observations I know in London are made by Cavendish, 1775 ·78; Sabine, 1821 ·62; Segelike, 1830 ·91; Lloyd, 1836 ·50; Phillips, Ross, Johnson and Sabine, 1837 ·63; Phillips and Fox, 1838 ·30; Sabine and Ross, 1838 ·74. As fifteen years have elapsed since the last observation, and I suppose many observations have been made in the mean time, I would be extremely obliged to you, if you will be so kind to communicate them to me, or *procure a good determination to be made in this year*. In reliance upon your interest for this inquiry, to which you have devoted so earnestly your labour and time in a long series of years, I hope you will not be displeased at my entreaty; and in this hope I remain, Sir, with great respect,

Your most obedient Servant,

CHR. HANSTEEN.

(I.) Inclination of the Magnetic Needle.

$$i = i_0 + (t - t_0)y + (t - t_0)^2 z.$$

	n.	Between	t_0.	i_0.	y.	z.	T.	Lat.	Long. East from Paris.
Paris	20	1798—1851	1800	69° 40'·80±1'·80	− 4'·2081+0'·175	+0'·01173±0'·00341	1978·7± 52·2	48° 50'	0° 0'
Brussels	23	1827—1852	1827	69 1·93±0·75	− 3·3954±0·122	+0·01993±0·00433	1912·2± 18·7	50 51	2 22
Berlin	12	1769—1837	1800	70 14·41±2·24	− 4·2737±0·056	+0·02166±0·00261	1898·6± 12·0	52 31	11 2
Copenhagen*	6	1820—1847	1820	70 39·23±1·41	− 2·8662±0·101	+0·03292±0·01182	1863·5± 18·0	55 41	10 15
Christiania	20	1820—1853	1820	72 41·44±1·19	− 3·3488±0·155	+0·03723±0·00429	1862·9± 5·6	59 55	8 23
Stockholm	8	1825—1853	1825	72 3·84±2·90	− 3·1268±0·408	+0·04897±0·01283	1856·9± 9·4	59 30	15 44
Petersburg	9	1830—1850	1830	71 11·62±3·06	− 1·9806±0·608	+0·04354±0·02758	1852·7± 16·0	59 57	27 59
Kazan*	5	1828—1843	1828	68 28·76±1·51	− 2·1065±0·062	+0·12181±0·05198	1836·6± 3·7	55 48	47 1
Catharinenburg ..	11	1828—1852	1828	69 42·56±1·30	− 0·0230±0·228	+0·03737±0·09010	1828·3± 3·0	56 50	58 14
Nertchinsk......	7	1832—1850	1832	66 30·19±2·30	+ 4·7983±0·187	−0·03855±0·02983	1855·1± 5·2	51 18	117 1
Pekin*	4	1831—1845	1831	54 47·62±1·55	+ 6·3065±0·359	−0·13666±0·02581	1854·1± 4·6	39 54	114 5
Gibraltar**......	4	1840—1847	1840	59 49·47±9·47	−11·346 ±6·103	+0·40193±0·6244	....	36 4	−7 34
London*........	7	1775—1838	1821·62	70 3·10±0·53	− 2·8549±0·00265	+0·00809±0·00081	1997·9± 18·1	51 31	−2 25
Göttingen*	5	1806—1842	1806	69 30·86±3·12	− 3·4688±0·1067	+0·01213±0·01106	1949·0±130·5	51 31	7 34
New York*......	4	1823—1846	1822	73 7·31±3·26	− 0·9484±0·5311	−0·00506±0·02038			
Sitka	4	1829—1845	1829	75 45·28±0·18	− 1·8704±0·1237	+0·15271±0·00747	1835·1± 0·5	57 3	222 25

(II.) i and $\frac{di}{dt}$ reduced to 1840·0.

	Lat.	Long. E. from Paris.	i.	$\frac{di}{dt}$.
	° ′	° ′	° ′	′
Paris	48 50	0 0	67 11·24	−3·2700
Brussels	50 51	2 22	68 21·16	−2·8772
London	51 31	−2 25	69 13·36	−2·5572
Göttingen	51 31	7 34	67 46·83	−2·6434
Berlin..........	52 31	11 2	67 58·12	−2·5394
Copenhagen	55 41	10 15	69 55·07	−1·5495
Christiania	59 55	8 23	71 49·36	−1·8597
Stockholm	59 30	15 44	71 27·96	−1·6578
Petersburg	59 57	27 59	70 56·17	−1·1098
Kazan..........	55 48	47 1	68 21·02	+0·8169
Catharinenburg ..	56 50	58 14	69 47·67	+0·8739
Nertschinsk	51 18	117 1	67 6·11	+4·1819
Pekin	39 54	114 5	55 33·11	+3·8466
Sitka	57 3	222 25	75 46·16	+1·4892
New York	40 43	283 31	72 48·27	−1·1307

These variations are in good harmony with the general motion of the magnetical system from west to east in the northern hemisphere. C. H.

3. A letter was also read from Dr. Rigby addressed to the Secretary, communicating a circular from the Committee of the Seckenberg Society of Natural History at Frankfort, respecting the celebration of the 50th Anniversary of Prof. Tiedemann's doctorate.

February 2, 1854.

COLONEL SABINE, R.A., Treas. & V.P., in the Chair.

The following papers were read:—

1. "Sur la Théorie de l'orientation du Plan oscillatoire du Pendule simple, et son application à la recherche de l'aplatissement du sphéroide terrestre." By M. Oliveira. Communicated by Charles Babbage, Esq., F.R.S. &c. Received January 18, 1854.

In this memoir the author first deduces a formula upon *geometrical* considerations *alone*, expressing the deviation of a free pendulum (like Foucault's) in terms of the latitude and difference of meridians, or hour-angle; and this is done (as far as appears) without any reference to the *dynamical* considerations on which Foucault's formula is deduced, assuming only the inertia of the pendulum.

The author's formula assumes the earth to be a *sphere*. If now, observation should give a slightly different deviation, the author infers that this would be due to the *ellipticity* of the earth; and in-

vestigates a formula geometrically, to express the ellipticity in terms of such difference; and thus by accurate observations of Foucault's pendulum in different parts of the earth, he conceives the ellipticity might be determined.

As an instance, he cites Foucault's result for the latitude of Paris; which differs by a small amount from the formula, and which he considers accordingly to express the ellipticity, though he does not calculate it.

2. "On the Extension of the value of the Base of Napier's Logarithms; of the Napierian Logarithms of 2, 3, 5, and 10; and of the Modulus of Briggs's, or the Common System of Logarithms; all to 205 places of decimals." By William Shanks, Esq. Communicated by G. B. Airy, Esq., Astronomer Royal, F.R.S. &c. Received January 21, 1854.

The author, after referring to the value of π to 527 decimals computed by him and printed in the 'Proceedings,' for January 20, 1853, states that he has very recently extended and computed the values which form the subject of this communication to 205 places of decimals; and as very great care has been taken to exclude error, it is presumed there exist reasonable grounds for pronouncing them quite accurate. At the same time it should be distinctly understood, that *no direct check or proof* has yet been applied to the values in question. He states that the formulæ employed in finding these logarithms, are investigated by Mr. J. R. Young, in his 'Elementary Essay on the Computation of Logarithms,' pp. 13 and 14, and he considers that no better formulæ than these have yet been published for readily computing, *to a great extent*, the Napierian logarithms of 2, 3, 5, 7, &c.

Subjoined are the values referred to :—

Base of Napier's Logarithms =

2·7182818	2845904	5235360	2874713	5266249
7757247	0936999	5957496	6967627	7240766
3035354	7594571	3821785	2516642	7427466
3919320	0305992	1817413	5966290	4357290
0334295	2605956	3073813	2328627	9434907
6323382	9880748	2070767	3049394	92 + &c.

Napierian Logarithm of 2 =

·6931471	8055994	5309417	2321214	5817656
8075500	1343602	5525412	0679523	5847083
2754439	2266635	5206804	5602137	0371911
8226310	4298719	4582110	0448886	1731607
5101002	4259177	6434321	7424545	3493150
3980048	7339123	6947695	8281006	80 + &c.

Napierian Logarithm of 3 =

1·0986122	8866810	9691395	2452369	2252570
4647490	5578227	4945173	4693570	0667031
1626456	2261348	7915959	6453630	4663543
4230252	7148232	3776931	0688498	5615669
0906550	5814573	8582278	9682167	2037498
0000626	1111154	1362298	9315024	24 + &c.

Napierian Logarithm of 5 =

1·6094379	1243410	0374600	7593332	2618763
9525601	3542685	1772191	2646780	8257554
5759268	0738412	2078288	5798574	2982618
5124170	8082338	1773353	3644800	7450601
6314333	5570584	1878072	7874564	5612567
3804931	0408586	1451680	3463508	54+&c.

Napierian Logarithm of 10 =

2·3025850	9299404	5684017	9914546	8436420
7601101	4886287	7297603	3326304	4104637
8513707	3005047	7285093	1400711	3354530
3350481	2381057	6355463	4093686	9182209
1415335	9829761	8312394	5299109	9105717
7784979	7747709	8399376	1744515	35+&c.

Modulus of Common System of Logarithms =

·4342944	8190325	1827651	1289189	1660508
2294397	0058036	6656611	4454084	2952103
2056138	9388912	2647096	6953461	1420043
3938056	4705613	4312230	2306044	2927744
1521725	4737266	8184290	1672329	4707564
5865061	2932297	5502468	4291564	99+&c.

The foregoing values are, it is presumed, correct to the last figure inclusive.

February 9, 1854.

SIR FREDERICK POLLOCK, M.A., V.P., in the Chair.

A paper was in part read, entitled "Further researches into the properties of the Sulphate of Iodo-Quinine or Herapathite, more especially in regard to its Crystallography, with additional facts concerning its optical relations." By William Bird Herapath, M.D. Communicated by Golding Bird, M.D., F.R.S. Received Jan. 27, 1854.

February 16, 1854.

COLONEL SABINE, R.A., Treas. and V.P., in the Chair.

Joseph Beete Jukes, Esq., was admitted into the Society.

The reading of Dr. Herapath's paper was resumed and concluded.

After referring to the observations of Professors Stokes and Haidinger, as well as to papers already published by himself on this subject in the Philosophical Magazine, the author gives an account of a set of prisms perfectly complementary in their optical characters to those previously described by him, and proves this fact by an elaborate comparison of their various optical relations; from which it appeared, that whilst the α-prisms (those described in Philosophical Magazine for March 1852) were totally impervious to a beam of polarized light, reflected from glass plates, when the plane of the length of the prism was at right angles to the plane of primitive polarization, the β-prisms (those now examined) were equally ab-

sorbent of a vertically polarized beam, when the plane of their length lay parallel to that of primitive polarization.

These prisms may be readily made by the following process :—

Dissolve 10 grains of disulphate of quinine in half a fluid ounce of spirit of wine, having 3 grains of benzoic acid dissolved in it also; adding 2 drachms of water and warming the whole to complete solution, then upon adding a few drops of spirituous solution of iodine and placing in repose, prismatic crystals having the following properties are produced:—

1. When two are crossed in superposition at right angles to each other or even at 30°, the overlapped space is "black" if the crystals are sufficiently thick, but reddish brown or violet-red if very thin; in these respects they perfectly coincide with the α-prisms.

2. When examined by polarized light reflected from glass, the *vertical* crystals now appear black, whilst the horizontal prisms are almost perfectly transparent to the polarized beam.

3. Upon introducing the selenite stages beneath the prisms, those which previously obstructed the polarized beam and appeared "black," transmitted the "red" or "blue" colours of the selenite films, whilst the transparent, horizontal prisms, exhibited the complementary colours, viz. the green or yellow.

The last two completely decided their complementary character, for whilst the β-prisms were "black," "red" or "blue," when the planes of their length were *vertical*, the α-prisms were "black," "red" or "blue" whilst the planes of their length were *horizontally* placed.

4. But by mixing the two prisms upon one slide their perfectly complementary character was completely demonstrated, for when an α-prism was crossed at right angles by a β-prism, the square space where they overlapped was still transparent to common light; but if a β-prism were overlapped by an α-prism, the length of the two being parallel to each other, the oblong space where they overlapped became black from their doubly absorbent properties; in short, the optical examination fully proved the complementary character of these crystals length for length.

It was further shown that two varieties of hexagons existed; those produced from strong spirituous solutions were longer than broad, and possessed two acute angles of 65° and four equal angles of 147° 30′. Examined by a vertically plane-polarized beam, these were "black" when a line passing through the long diameter or acute angles was parallel to the plane of primitive polarization: these the author called "acute" or β-hexagons. The hexagons produced from acetic acid solutions, on the other hand, were "obtuse" in their character, and had two angles of 115° and four equal angles of 122° 30′. These, when examined by vertically plane-polarized light, were always "black" when a line passing through the two angles of 115° was perpendicular to the plane of primitive polarization: the author called these "obtuse" or α-hexagons.

The rhombic crystals which presented themselves were also "black" (examined in the same way) when their long diameters lay parallel

to the plane of primitive polarization, but transparent when they were rotated 90°.

It was also shown that the doubly absorbent powers of the primary rhombic crystal extended more or less on each side of the vertical position, through an arc of 60°, so that upon rotating a crystal through the whole circle, there were two arcs of 120°, in which the crystals were more or less darkened, and two of 60°, each in which the light passed through without loss; this transparency was of course at its maxinum when the longer diameter of the rhombic plate lay in a horizontal position, and gradually became less evident through an arc of 30° above and below this line. By a careful measurement of the angles of the various crystalline forms and by the results of their optical examination, it appeared that they may all be obtained from the right rhombic prism, the acute angle of which is 65° and the obtuse 115°, the major axis being to the minor as 1·57 to 1, whilst the prism itself had scarcely appreciable length. It was a mere rhombic scale; but considered as a short prism, the axes were $P_{\frac{a}{0\cdot00001}}$, $M_{\frac{a}{1\cdot57}}$, $T_{\frac{a}{1\cdot0}}$, the quantity for P^a not being absolutely determinable in consequence of its variability and minuteness.

It appeared also that the α-prism and obtuse hexagon were the results of truncation of the *acute* angles of the primary rhombic plate or prism by planes at right angles to the plane of primitive polarization, when the crystals were "black," when examined by vertically plane-polarized light; whilst the β-prism and acute hexagon resulted from truncation of the *obtuse* angles of the same primary form by planes parallel to that of primitive polarization, the crystals being "black" when examined by a vertically plane-polarized beam; and that the octagonal and square plates, and rectangular parallelogram resulted from the coincidence of these truncation planes in the same individual form.

It was shown that the solvent medium had the power of developing these truncation planes, and it appeared that *water* produced the α-truncation, and *spirit* the β-truncation; and if the two opposing forces of water and spirit were made equal in intensity, they neutralized each other, so that the pure primary rhombic prism resulted without truncation.

It was further shown that hyponitric ether developed other crystalline forms, converting the rhombic plate into one of 75° and 105° by truncation planes upon the acute angles of the primary rhombic plate, cutting off portions equal to half the long axis, and leaving the shorter or transverse axis untouched. By this means the new rhombic crystal appeared "black" when the longer diameter was at right angles to the plane of vertically polarized light, as if it had been rotated through 90°, whilst it absorbed the polarized light as before.

As from the examination of certain rectangular quadrilateral prisms of the α and β varieties, it appeared that Herapathite possesses doubly absorbent powers of nearly, if not perfectly equal intensity, in directions coincident with all three rectangular axes P^a, M^a and T^a, the author inferred that the development of their optic

axes could not be reasonably expected in these directions. He considered that the biaxial systems of rings would be found to exist in thicker crystals (when discovered), having triangular replacement planes upon the solid angles, either of the shorter or longer oblique diameters of the right rhombic prism, and the results of these optic axes would be seen in a direction perpendicular to these surfaces of replacement.

The various formulæ necessary to produce different crystalline forms are given in detail.

The author enters into a re-discussion of the double refractive powers, and attempts to show that Herapathite possesses a principal axis having "positive" characters. He also endeavoured to measure the thickness of certain very thin plates, by the differential tints produced by the exercise of their doubly refracting powers upon the colours of the selenite stages, and found that these thin plates raised the blue of the second order to the yellow and violet; the red of the second to the violet and blue-grey; and the green of the same to orange and violet of the second order. Assuming, therefore, this substance to have similar doubly refracting powers to selenite, and of equal intensity, and also the same index of refraction, their thickness may be estimated as equal to that of a plate of selenite necessary to give the same difference in tint, viz. ·000517 of an inch.

These very thin plates no longer possessed the Cantharides-like or brilliant metallic green colour when examined by reflected light. They appeared more like portions of the elytra of the *Blatta germanica*; still reflecting a polarized beam, but of a brown colour, and also retaining their doubly absorbent powers when examined by transmitted light.

The author considers that his recent investigations indicate that whatever properties Professor Stokes may have assigned to the ray reflected from the α-prisms (which alone were experimented on by him) in the principal plane of the breadth, must be equally true in that reflected from the primary rhombic prism in the principal plane of its length, *i. e.* its long diameter; and whatever properties he may have assigned to the reflected ray from the principal plane of the length of the α-prism, will be equally true of that reflected from the primary rhombus in the principal plane of the breadth, *i. e.* its short diameter.

The paper concluds with some recently determined facts relating to the chemical characters of this peculiar substance.

Its sp. gr. at 60° Fahr. was found to be 1·895.

It is very sparingly soluble in ether, turpentine or water at the ordinary temperatures, scarcely more than $\frac{1}{2000}$th part; boiling water does not dissolve $\frac{1}{1000}$th part. It does not appear more soluble in ether or turpentine by boiling. Chloroform does not dissolve it, but has a great attraction of surface for it.

Alcohol of ·837 dissolves $\frac{1}{650}$th part at 57° Fahr., and by boiling, it takes up $\frac{1}{50}$th part, which readily crystallizes on cooling in β-hexagons.

Acetic acid, sp. gr. 1·042, dissolves $\frac{1}{750}$th part at 60° Fahr., and

$\frac{1}{60}$th part at boiling temperature—iodine volatilizing if the heat is prolonged.

Diluted sulphuric acid, sp. gr. 1·0682, does not dissolve it at the ordinary temperatures, but does so readily with the aid of heat. *Sulph. acid*, sp. gr. 1·845, rapidly dissolves it.

Diluted hydrochloric acid has but little action on it, but when concentrated, it almost instantly reddens, without dissolving it; upon boiling, it forms a yellow solution, from which minute dark opake radiating aciculæ deposit on cooling.

Nitric acid immediately decomposes it even in the cold: upon raising the temperature, iodine at first volatilizes; then nitrous acid vapours are evolved. Iodine is probably partially converted into iodic acid.

Hydro-sulphuric acid passed through its alcoholic or acetic acid solution at once decomposes it, converting the iodine into hydriodic acid, with separation of sulphur. .

Alkalies and alkaline earths in solution at once decompose it, removing the sulphuric acid and leaving a Naples yellow residue containing the quinine and a portion of the iodine; a soluble iodide of quinine is also formed in ammoniacal liquids.

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END OF THE SIXTH VOLUME.

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